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I. WHAT DETERMINES MOLECULAR MOTION?—

THE FUNDAMENTAL PROBLEM OF NATURE.

CAN the whole phenomena of nature be explained in terms of *matter*, *motion*, and *force?* Yes, say the materialists; and the number of men of science who agree in this view is not only large, but is increasing daily, whilst a doctrine which would scarcely have been tolerated some years ago is now openly maintained by some of the leaders of scientific opinion. A year ago, Mr. James Croll, of the Geological Survey of Scotland, published some arguments which arrange the problem in an entirely new light; and we are indebted to the courtesy of the author for a private copy of his paper, which was first published in the "Philosophical Magazine," and from which we condense the following arguments.

Hitherto, the chief enquiry has been to ascertain the laws which govern the motions of the molecules of matter, but this is not the grand and fundamental problem. It can be subdivided into—(1.) What produces the change—causes motion? and (2.) What determines or directs it?

The answer to the first question is Force, but the second question—not only the more difficult of the two, but also by far the more important—has been lost sight of or confused with the first. Physicists have devoted almost exclusive attention to the study of the force which takes the path of light, heat, electricity, &c., and upon what does its exertion depend, whilst a vastly more important problem, "What is it that causes the force to act in the particular manner in which it does act?" In other words, "What determines the paths along which it acts?" The great question is, not what gives *existence* to the motion, but what *determines its direction.*

Arguing in this manner, Mr. Croll is led to the proposition that—*The production of motion and the determination of motion are absolutely and essentially different.*

In physics we have been accustomed to attribute every thing to force; force, at least, has always been regarded as the all-important element. This, however, is a mistake; for, as we shall see, far more depends upon the *determination* of force than upon its *existence*, and therefore, *unless force be determined by force, the most important element in physical causation is a something different from force.*

Motion is not only produced, but it is produced in a particular manner and under particular conditions or determinations in regard to time and space and other circumstances. In other words, not only must something produce the motion, but something must determine it also. The causing of, or giving mere existence to the motion, Mr. Croll has called the *production* of the motion. The causing of it to happen in the particular manner in which it does, rather than in some other manner, he calls the *determination* of the motion. It must be evident to every one who will consider the matter that these two things are radically distinct. And they are not only radically distinct, but must be separately accounted for. To account for the mere existence of motion does not account for its happening in one way rather than in some other. It is quite true that the one cannot be produced without the other; we cannot determine motion unless there is motion to be determined; we cannot determine that which has got no existence; neither, on the other hand, can we produce motion without at the same time giving it some particular determination in regard to time, place, or other circumstance. But, although the one cannot be produced without the other, yet they are the result of different agencies; and to assign a sufficient cause for the one does not in the least degree satisfy the mind as to the presence of the other. To account for the motion of a ball does not account for why it moves, say, east rather than west, or in any other possible direction. A force, it is true, cannot act without at the same time acting in some particular way, nor move a body without moving it in some particular direction; but to account for the one does not satisfy the mind in regard to the other. The explosion of the powder within a gun is a sufficient cause for the motion of the ball, but the explosion of the powder is not to the mind a sufficient cause why the ball moves east rather than west, or in any other direction.

The grand and fundamental question then is, What is it that determines or directs the action of the forces concerned in the production of molecular change? The question therefore regards not Law but Cause, unless we use the term law in an improper sense. Law in physics is not an agent or force, it is simply the *process* or mode of operation —not the force, but the path along which the force acts.

A prodigious number of physical phenomena are perceived to follow as necessary consequences from Newton's grand law, that bodies tend toward each other with a force varying inversely as the square of the distance and directly as the mass of the bodies. But we should reach a higher unity and obtain a deeper insight into nature did we know not merely the empirical fact that bodies do so, but the cause why they do so. It is this which incites in the rational physicist the desire to find out the cause of gravity,

But be all this as it may, whether it be Cause or Law, that is the thing which we are really in search of, every one will admit that the problem of deepest interest is, what causes the molecules and particles of living nature to arrange themselves into organic forms? The problem is not what moves the particles, but what determines or directs the motion—or, in other words, what is the cause of the determination of motion?

What, then, determines molecular motion in organic nature? What determines and directs the action of the forces concerned in the production of specific forms in the inorganic and organic world? Is it a Force? This leads us to Mr. Croll's second proposition, viz.—*The action of a force cannot be determined by a force, nor can motion be determined by motion.*

That the action of a force cannot be determined by the action of a force is demonstrable thus. If the action of a force is determined by an act, then this determining act must itself have been determined by a preceding act; and this preceding act by another, and so on in like manner to infinity. This is evident; for if the act which determines the action of the force exist at all, it must exist in time and space, and must have a determinate existence in reference to time and space, and if so, something must have given it that determinate relation. If it be replied that it was a prior act which determined this determining act, then that prior act in order to give the determining act the proper determination must itself have been already properly determined; and the question again recurs, what gave this prior act the proper determination? If the determination was given by an act still prior, that act must itself have been properly determined; and if so, then there must have been another act preceding which gave it the proper determination, and so in the like manner to infinity. The reason of all this is perfectly obvious. When we account for the determination of an act by assigning an act, we account for it by means of a something which requires itself to be accounted for in a similar manner.

Hence, be the cause of the determination whatever it may, it cannot possibly be an act or exertion of force.

In a similar manner we can prove that motion cannot be determined by motion. Motion will produce motion, but motion cannot determine motion. A ball A in motion will produce motion in a ball B, but the motion of the ball A will not determine the motion of the ball B, either in regard to direction or to the times of its happening. The particular direction taken by the ball B is not due to the motion of A, but to the particular direction in which A is moving at the moment in which it produced motion in B; so that the direction taken by B must be referred not to the motion of A, but to that something, whatever it may be, which causes A to move in the particular direction in which it moves. In other words, the determinate direction taken by B is not due to the motion of A, but to the *direction* of the motion of A. In like manner it can be proved that the direction taken by A is not due to the motion of some other body (say C), but to the *direction* of that moving body C.

In a similar way we can prove that the particular time at which B begins to move is not due to the motion of the striking body A, but to the *particular time* at which the body A strikes B.

The vague and indefinite idea that the arrangement of the molecules of matter into crystalline and organic forms is due to the action of forces, appears to be implied in such terms in common use as "structural forces," "formative forces," "crystal-building force," &c. It is supposed that if our mental powers were enlarged or strengthened so that we could perceive every thing connected with the forces operating in nature, we should then be able to explain the process by which the organic forms of nature are built up. This, however, is evidently a mistake. Though our acquaintance with the forces of nature were absolutely perfect, the question as to how particles or molecules arrange themselves into organic forms would probably still remain as deep a mystery as ever, unless we knew something more than force.

The mystery is not what are the forces which move the particles, but what is it that guides and directs the action of the forces so that they move each particle in the particular manner and direction required. Force gives motion to the particles; but we are not concerned about the cause of the motion, but about what *directs* that motion.

When a molecule is to be moved, there is an infinite number of directions in which force may be conceived to move it. But out of the infinite number of different paths, what is it that directs the force to select the right path?

Is it asserted that force is self-directing? This is simply getting into confusion again. What conceivable idea can be attached to a self-directing force? Is force a something which not only acts but determines for itself how and when it shall act? In what conceivable way can force direct its own path? A molecule has to be moved into its proper place in an organic form; a force gives motion to the molecule; but out of the infinite number of possible directions in which the molecule may be moved the force moves it in the right direction. What is that something which thus guides the force? The force guides itself, it is replied. Be it so; but in what way does the force direct or guide itself? What is the nature of that something in virtue of which the force directs its actions? Is it supposed that that something belonging to the force which thus guides and directs its action is itself a force? Does the force direct itself by means of a force? if so, then we are back to our old absurdity of a force determining a force. And if this directing something is not a force, what is it? But if this something is not a force, it follows that there is something else to be known than mere force before we can penetrate the mystery of nature.

The simple truth is, in attempting to account for the *determination* of motion by referring it to a *force*, we are attempting an absolute impossibility. The *production* of motion and the *determination* of motion are two things absolutely different in their essential nature. Force produces motion; but it is as impossible that force can determine motion as that two can be equal to three, or that a thing can be and not be at the same time. The necessity is as absolute in the one case as in the other.

If any one imagines that he can conceive motion as being directed or determined by a force, he will find, on subjecting his thoughts to a proper analysis, that the determination is not due to the force which he imagines, but is due to the *direction* in which his imagined force exerts itself. The determination results not from his imagined force, but from the *way* in which his force acts.

As the distinction between the production of motion and its determination, or between the production of an act and its determination, is absolute, it must hold equally true in the mental world as in the physical. For example, it is just as impossible to conceive the will being determined by an act, as to conceive the motion of the cannon-ball being determined by the explosion of the powder. It is difficult to say whether in physics or in metaphysics the distinction is of most importance.

What is the cause of determination? What is that something which determines the energies of the universe and guides the motion of the material particles? This is the all-important question, whether as regards life-theories, theism, or evolution.

To a large extent the discussions and diversity of opinion which at present prevail in reference to the mystery of life and the distinction between the organic and the inorganic world take their rise from confusion of ideas regarding the difference between the cause of motion and the cause of the determination of motion. The various theories may be divided into two classes,—the advocates of the one class maintaining that all the phenomena of life, all the changes which take place in organic nature, are the result of purely chemical and physical agencies; while the other party maintain that there must be something more than the ordinary chemical and physical forces at work—in short, that life and organic nature imply the action of a force altogether different from those which belong to the domain of chemistry and physics, and to which the name of "*vital force*" has been applied.

Evidently the vital energies of the plant and animal are derived from the chemical affinities of the food and nutriment which they receive. Vital force is chemical force transformed. The same remark holds true of the mechanical and other physical energies of the body. The energy by which the arm is raised or by which the heart beats is derived from the food. Animal heat is derived from chemical combination.

So far as all this is concerned, the advocates of the physical theory of life are evidently correct. But are they warranted in affirming, as they do, that all the energies of plants and animals are either chemical or physical? Whether such an affirmation be correct depends entirely on the idea which may be attached to the terms chemical and physical.

When the advocates of the physical theory of life affirm that every energy in organic nature is either chemical or physical, they certainly do not mean to include under the term physical every form of energy which does not, like chemistry, deal with the elementary substances; for if this were their meaning, it would simply be a truism to say that all energy is either chemical or physical. By physical energy they undoubtedly mean the ordinary and known forms of energy manifested in the inorganic world, to which we give the various specific names of attraction, repulsion, light, heat, electricity, magnetism, and so forth. But here we now approach the real question at issue, viz., are these forms of energy along with chemical energy sufficient to account for the phenomena of life and organic nature?

Chemistry and physics are insufficient, because they do not account for the *objective idea in nature.*

Whatever may be one's opinions regarding the doctrine of final causes and the evidence of design in nature, all must admit the existence of the objective idea in nature. We see everywhere not only exquisite order and arrangement in the structure of plants and animals, but a unity of plan pervading the whole. We see, in endless complexity, beauty, and simplicity, the most perfect adaptation of means to ends. The advocates of the physical theory are at least bound to show how it is probable that this exquisite arrangement and unity of plan could have been produced by means of chemical and physical agencies.

Natural selection never can explain the objective idea in nature unless we suppose the selection to be made according to a design or plan. Mr. Darwin has developed a new and most important idea; but his theory can never, from its very nature, explain the mystery of the organic world. There must be a *determining* cause in the background of all natural selection working out the objective idea. This I trust will be rendered more evident when we come to consider determination of motion in relation to final causes.

Mr. Croll now considers the explanation of molecular motion in regard to the *form* of objects.

The objects of nature, as we have seen, are built up molecule by molecule, and are thus the products of molecular motion. Energy is that which moves or transports the molecules in the building-up process; but it is not the mere transport of the molecules, as has been repeatedly shown, which gives to the object produced its form. The form assumed is due, not to the motion of the molecules, but to the determination of that motion—to the way in which the motions are guided and adjusted in relation to one another. It is not the energy which conveys the bricks that accounts for the form of the house, but that which guides and directs the energy. So far as the form of the house is concerned, it is a matter of indifference whether the bricks are conveyed on the backs of labourers or transported by a steam-crane. In like manner, in accounting for organic forms, we must exhibit not the mere energy which moves the molecules, but that which directs and guides the energy.

But it has been already proved that energy cannot be determined by energy: consequently that which determines energy is not itself an energy. Therefore the thing which we are in search of, which accounts for the order and arrangement prevailing in the molecular movements in nature, is a something not of the nature of a force or an energy.

The question now to be considered is, Can this marvellous adjustment of molecular motions be explained by any thing which is found within the domains of chemistry and physics? The advocates of the physical theory must afford us some explanation of the cause of the determination of molecular motion derived from physics and chemistry, if their theory in reality rests upon a true foundation.

Energy, chemical and physical, accounts for molecular motions in organic nature; but how is it to account for the determination of those motions? If the determinations of molecular motion are to be attributed to these energies, it must be to their modes of operation—the *way* in which the energies are exerted—and not to the mere exertion itself. Suppose that the determinations of molecular motion could be accounted for from the known *modes* of the operation of physical energies. The ultimate problem would then be, What is it that determines those modes of operation? In other words, the problem would resolve itself into this, viz., What is the cause of the determination of physical energies? What is it that directs the operation of those energies?

Molecular physics has made great advance of late years; but it has not made much advance in that particular direction which can be of service in explaining how molecular motion in organic nature is determined. It is thought, however, by the advocates of the physical school that although at present we are unable to explain how organic nature can be built up by the play of the ordinary chemical and physical forces, yet at some future day, when we shall have come to know far more of molecular physics than we do at present, then we may be able to explain the mystery. This is the cherished hope of modern evolutionists, and of the advocates of the physical theory of life. But it is a mental delusion, a dream which will never be realised. A little consideration might satisfy any one that chemistry and physics will never explain the mystery of nature.

It must be obvious that nothing which can be determined by the comparative anatomist, no biological researches, no microscopic investigations, no considerations regarding natural selection or the survival of the fittest, can solve the great problem of nature; for it lies in the background of all such investigations. The problem is molecular. From the hugest plant and animal on the globe down to the smallest organic speck visible under the microscope, all have been built up molecule by molecule; and the problem is, to explain this molecular process. If one plant or animal differs from another, or the parent from the child, it is because in the building-up process the determinations of molecular motion were different in the two cases; and the true and fundamental ground of the difference must be sought for in the cause of the determination of molecular motion. Here in this region the doctrine of natural selection and the struggle for existence can afford no more light on the matter than the fortuitous concourse of atoms and the atomical philosophy of the ancients.

The next part of Mr. Croll's paper, the publication of which has been unavoidably delayed, will consist of an examination of the arguments which have been advanced by evolutionists in support of their fundamental hypothesis, "that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces possessed by the molecules of which the primitive nebulosity of the universe was composed."

II. SOME NEW FACTS CONCERNING THE DIAMOND.

WHILST our knowledge of the modes of formation of other gems is so rapidly advancing that the time does not seem to be very distant when the chemist in his laboratory will be able to produce them artificially—if not in large, at all events in microscopic crystals,—the origin and mode of formation of the diamond is shrouded in apparently inexplicable mystery. It is even undecided whether the diamond is of igneous or vegetable origin, whether its nature is mineral or organic: some diamonds appear to have been soft, as they are superficially impressed by sand and crystals; others contain crystals of other minerals, germs of plants, and fragments of vegetation. Professor Goppert has a diamond containing *dendrites*, such as occurs on minerals of aqueous origin; and there is at Berlin a diamond which contains bodies resembling *Protococcus pluvialis*, and another containing green corpuscles linked together closely, resembling *Polinoglæa macrococca*. Sir John Herschel quotes the case of a Bahia diamond, mentioned by Harting, which contained well-formed filaments of iron pyrites. Messrs. Sorby and Baker have shown that the diamond may contain cavities entirely or partially filled with a liquid, probably condensed carbonic acid, and that the black specks in diamonds are really crystals which are sometimes surrounded by contraction cracks, a black cross appearing under polarised light. Sir David Brewster has likewise pointed out that the diamond possesses strata of different refractive powers. M. Damour states that diamonds sometimes contain spangles of gold in their cavities.

The latest researches published relative to the diamond, namely, those of M. Schrötter and M. G. Rose have furnished results which in some respects correspond neither with each other nor with the facts already known to science. M. E. H. von Baumhauer has instituted some experiments on the subject, special attention being paid to the different states in which the diamond occurs in nature. This talented experimentalist having courteously placed a copy of his research at our disposal, we are enabled to give the most important of his results in these pages. In the first place it was proposed to ascertain the density of the diamond in each of its natural states, afterwards to study its behaviour at high temperatures in the presence of various gases, and lastly to attempt the decision of a point as yet unsettled, namely, the possibility of its transformation by means of heat, into graphite or amorphous carbon. The materials necessary for experimentally pursuing these inquiries were furnished by the liberality of M. Alexander Daniels, the talented manager of the diamond-cutting establishment at Amsterdam, belonging to M. Martin Coster, of Paris.

The condition of a more or less perfect crystal, transparent and colourless, or nearly so, is by no means the only one under which the diamond is found, although for some time it was only known and sought for in that form. At present, equal attention is paid to irregular fragments of a blackish or greyish colour, occasionally of considerable size, also yielded by the washings of diamandiferous sand, which formerly passed unregarded. These fragments are now carefully collected, and have acquired some considerable value in commerce, where they are known under the name of *carbonado* or carbon. Their aspect is generally that of a rounded mass (although they are sometimes angular) blackish in colour, and presenting a shiny surface as if polished by rubbing. When split, however, their appearance is dull, relieved here and there by a brilliant speck, numerous pores of unequal dimensions being perceptible with a lens. The colour of the split surface varies, being sometimes greyish and sometimes violet. Upon heating in water, considerable evolution of gaseous bubbles ensues; it is therefore necessary, when ascertaining their specific gravity, to boil the fragments for some time in water, to liberate as much as possible the air contained in their pores. Upon treatment with *aqua regia* the solution was found to contain a considerable quantity of iron and a small portion of lime, but no traces of either sulphuric acid or alumina were to be found. Combustion in oxygen produced a small quantity of ash from 0·24 to 2 per cent, according to Rivot. It is to be hoped that *carbonado* may be subjected at some future time to a more minute examination.

Although quite unadapted to ornamental purposes, owing to its appearance, which is totally dissimilar to that of the diamond, *carbonado* cannot be considered as constituting an essentially different condition. Upon examining large quantities of *carbonado* and of diamonds, it is often difficult to decide whether certain specimens should be placed in the category of *carbonado*, which presents no appearance of crystalline structure when seen by the naked eye, or in that of dark diamonds of incomplete and irregular crystallisation. An examination of these numerous varieties has made it evident that between *carbonado* of a simply micro-crystalline texture, and the diamond regularly crystallised in diaphanous octahedrons, there exists an uninterrupted series of intermediate conditions. The true diamond will cleave in a direction parallel with the facets of the octahedrons, while pure fine-grained *carbonado* possesses no such quality, which may nevertheless be found in various degrees amongst the intermediate varieties. It is a remarkable fact that *carbonado*, which in Brazil, and especially in Bahia, always accompanies the diamond, in fragments whose weight sometimes amounts to several decagrammes, has not yet been discovered at the Cape of Good Hope, although the attention of the diamond seekers has been especially directed towards this apparently worthless substance; it may be therefore inferred that it does not exist in the diamandiferous alluvium of that locality. An examination of two small black fragments, sent from the Cape as supposed *carbonado*, showed that they contained no *carbonado* whatever, but consisted almost entirely of hydrated oxide of iron.

In addition to *carbonado* and to the ordinary diamond, there exists another variety, distinguished by lapidaries under the name of *boart*. The appearance of this is usually spheroidal, translucid—but not transparent—and colourless, or of a greyish tint: it cannot be converted into octahedrons by cleavage, and, moreover, it is much harder than the crystallised diamond, yielding, however, in this respect to *carbonado*. On account of their greater hardness, *carbonado* and *boart* are employed almost entirely in the composition of the powder used in diamond cutting, such powder being greatly preferred by the lapidaries to that formerly in use, which was obtained from well crystallised diamond.

It was thought that crystalline boron, which for some years has been obtainable by artificial means, would equal or even surpass the diamond with respect to its hardness, and might therefore be applicable to the working of diamonds, but the question remains an open one; the high price of the substance in question being opposed to its employment.

As regards the density of the diamond, many data exist, which, however, show many discrepancies. The number 3·5295 was observed by Thompson, at a temperature which is not indicated; M. Halphen found the density of the celebrated diamond, known as the "Star of the South," to be 3·529 at 15° C. M. Schrauf, operating upon the Florentine diamond belonging to the crown of Austria, obtained the number 3·5143, as a mean of two experiments whose results corresponded but little, and after correction for the weight of air, water at 4° C. being taken as unity. A series of experiments were made by M. Schrötter upon different diamonds, among which were always several which were at the same time coloured, and imperfectly transparent, and others which were traversed by fissures. After making the necessary corrections for the weight of the air, the average density of the diamond was found by this gentleman, when compared with that of water at 4° C., to be 3·51432.

Profiting by the facilities offered by M. Daniels, a series of experiments upon density were effected by M. von Baumhauer with all precautions required by this operation.

The water used in the weighing was boiled and cooled in vacuum, the temperature of the water and also that of the air being noted. During the weighing of the diamond in the air upon the pan of the balance, the platinum boat, which was to receive the diamond for its subsequent weighing in water, was suspended to a human hair and already plunged into the liquid, so that the determination of its absolute weight and that of the loss in water should be made under the same conditions, and consequently with the same degree of accuracy. The pressure of the air having varied only between 759 and 761 m.m. during these experiments, it was considered unnecessary to correct for variation of the barometer, such correction exercising no influence upon the result, since the exactitude of this mode of weighing in which there is friction of the hair against the water attains at least the demi-milligramme.

The subjoined table (see page 442) contains the result of these experiments; in the last column will be found the calculated densities D, obtained by adding to the numbers of the last column but one, the corrections arising from one of the weighings having taken place in water at the temperature t_1 and not in water at 4° C., and from the other weighing having been effected in the air at temperature t, and not in a vacuum.

This calculation was performed according to the known formula :—

$$D = \frac{P}{P^1} d_t^1 - \frac{P - P^1}{P^1} \; \frac{ab}{760(1 \times \beta t)},$$

where $a = 0{\cdot}00129337$ grm., is the weight of a cubic centimetre of air at 0° C., and 760 m.m. of barometric pressure, and $\beta = 0{\cdot}00367$ the coefficient of dilatation of the air; no correction having been made for the height of the barometer, $\frac{b}{760} = 1$ was taken.

On comparing the corrected densities in this table, it will be perceived that the purest diamonds possess the highest specific gravity.

No. 1	3·51821
„ 2	3·52063
„ 3	3·51727
„ 4	3·51631
„ 5	3·51934

No.	Quality and Colour of Diamond.	Temperature of Air, t.	Weight of Diamond in Air, P.	Temperature of Water, t^1.	Loss of weight in Water, P^1.	Uncorrected density, $\frac{P}{P^1}$.	Corrected density, D.
1.	Brilliant, nearly colourless (Cape) .	8·0	1·2569	8·0	0·3569	3·5217	3·51812
2.	Brilliant, pale yellow (Cape) . .	8·8	0·7059	8·0	0·2003	3·5242	3·52063
3.	Crude diamond, limpid yellow (Cape)	6·8	6·4830	5·2	1·8415	3·5205	3·51727
4.	Ditto, smaller, quite pure (Cape) .	9·0	1·7095	6·8	0·4857	3·5197	3·51631
5.	Ditto, small black spot in the interior (Cape)	5·5	1·7014	4·9	0·4830	3·5225	3·51934
6.	Ditto, large spot, and fissures (Cape)	9·0	1·1270	7·5	0·3214	3·5065	3·50307
7.	Rough diamond, limpid, consisting of two joined crystals. .	8·0	5·5774	7·0	1·5855	3·5178	3·51436
8.	Spheroid greyish *boart* translucid, not transparent	9·0	1·2808	7·5	0·3649	3·5100	3·50383
9.	White spheroidic *boart* (Cape) . .	14·0	3·9297	12·0	1·1202	3·5080	3·50329
10.	Ditto, smaller (Cape)	14·0	1·2292	12·5	0·3509	3·5030	3·49806
11.	Grey *carbonado* with slight violet tinge (Brazil).	13·0	17·5520	10·0	5·4780	3·2041	3·20053
12.	Greyish black *carbonado* (Brazil) .	13·0	10·3870	10·8	3·1506	3·2969	3·29287
13.	Black *carbonado* (Brazil)	13·0	7·2790	11·1	2·3070	3·1552	3·15135
14.	Spheroidal *carbonado* (Brazil) . .	13·0	2·4165	11·5	0·7215	3·3493	3·34497
15.	Ditto ditto ditto	13·0	1·0965	12·0	0·3418	3·2080	3·20378
16.	Grey, semitransparent, considered *carbonado*, but certainly crystalline	14·0	6·8982	11·5	1·9647	3·5111	3·50652
17.	White, semitranslucid, slightly crystalline, wrongly considered to be *carbonado*	13·0	2·3285	11·7	0·6640	3·5068	3·50215

To take the average of such results, which in the author's opinion is not permissible, the corrected density of the diamond, as compared with water at 4° C., would be 3·51835.

The omission of Nos. 6 and 7 is intentional, those diamonds being permeated with fissures, possibly containing air, which would alter their specific gravity.

It is considered that the density 3·51432, declared by M. Schrötter to be the lowest of his results, is too small; for, amongst the diamonds which he examined, some had blemishes or fissures, whilst in those which were without defect he also obtained much higher numbers; for instance, 3·51869 in a perfectly colourless diamond, 3·51947 in one of pale violet colour. Judging from all known results, M. von Baumhauer believes the density of a pure diamond should not be much less than 3·52.

The figures contained in the Table also show that the density of *boart*, or the globular diamond, seldom exceeds 3·50, whilst *carbonado* possesses a considerably lower specific gravity, being probably a porous diamond, a conclusion confirmed, moreover, by examination with a lens. The higher density found in Nos. 16 and 17 prove that these specimens are not *carbonado*, but some intermediate variety between that and the true diamond.

When shielded from contact with the air, the diamond may be exposed to the highest temperature of our furnaces without undergoing alteration, at least in the case of the colourless diamond; of coloured diamonds more will be said hereafter. The experiment by which this fact is generally demonstrated is conducted as follows: the diamond is placed in a small Hessian crucible and covered with closely compressed magnesia; this is introduced into another crucible which is completely filled up with well pressed graphite, and then the whole is subjected for a long time to the strongest heat obtainable in a porcelain furnace. This experiment was successively repeated by Morren, Schrötter, and others, who ascertained that, notwithstanding the excessive heat to which the diamond was subjected, it underwent no change either in shape or quality; Schrötter, however, remarked that the surface became slightly dull. Similar experiments were made at Berlin by the late Gustav Rose, with the co-operation of Dr. Siemens. A crystal of diamond, enclosed in a piece of dense coke and placed in a plumbago crucible packed with charcoal powder, was heated for half an hour in one of Siemens's regenerative furnaces to the temperature at which cast-iron melts without undergoing any change whatever. Another diamond, a cut (rose) diamond, which was enclosed in a crucible as before and heated for ten minutes in the furnace to a temperature at which wrought-iron melts, retained its form and the smoothness of its facets, but became quite black and opaque and exhibited a strong metallic lustre. The black portion formed a distinct layer of the thickness of a hair covering the unaltered substance within. These results confirm those of Schrötter, and appear to justify the view that diamond, though it undergoes no change when exposed to the greatest heat of a porcelain furnace or that at which cast-iron melts, is slowly converted at the temperature of molten wrought-iron into graphite.

G. Rose states that some of the specimens of diamond in the Berlin Collection appear quite black by reflected, though translucent by transmitted, light, and that this black substance lying in the little irregularities of the surface is found by its behaviour in fused nitre to be graphite. The relative ease with which graphite and diamond burn was determined by exposing them to the same temperature for the same time, when the following amounts of the three specimens mentioned below were consumed:—

Foliated graphite	27·45 per cent.
Diamond	97·76 ,,
Granular massive graphite . .	100·00 ,,

The method employed by M. von Baumhauer on examining the action of heat upon the diamond was as follows:—

After previous weighing, the diamond was placed in a small platinum crucible of an elongated and narrow form, similar to those recommended by Mr. J. Lawrence Smith for the decomposition of silicates by chloride of calcium. To enable the operator to observe what took place in the interior of the crucible, it was placed in an inclined position and closed by a thin plate of mica; an opening was pierced in this plate, through which passed a small thin tube of platinum, soldered at the other end to a glass tube connected with an apparatus which supplied hydrogen dried over sulphuric acid and chloride of calcium. By this means the diamond is surrounded by an atmosphere of dry hydrogen during the experiment. The crucible is heated to whiteness over a gas flame, intensified by a current of air. It was ascertained that the diamond, after exposure for fifteen minutes to a temperature in which it became invisible (that is, where the platinum and the diamond could no longer be distinguished from each other, from their mutual brilliancy), lost nothing of its weight after cooling, and retained all its transparency and brilliance of surface. The experiment was several times repeated upon colourless diamonds, or those of a pale yellow tint, but always with the same result; in an atmosphere free from chemical action upon it, the diamond may be subjected to a white heat for a considerable time without undergoing any change.

In a superb cut diamond weighing between 6 and 7 carats, the brilliancy of the stone was decidedly increased after the operation. The loss of brilliancy observed by M. Schrötter is a proof, in M. Baumhauer's opinion, that notwithstanding the precautions employed, the diamond had come in contact with the oxygen of the air, or else that at so elevated a temperature a reducing action had been effected upon the magnesia by the diamond, which had then been superficially burnt by the oxygen of that earth.

A diamond which presented to the naked eye an appearance of dirty green, was treated in a similar manner; examination with a lens showed that the colour did not extend to the entire stone, but was confined to small portions, which formed small green clouds in the centre of the mass. After heating to a white-heat in hydrogen, the brilliancy of surface remained as before; the transparency was rather increased than diminished, but the green hue was transformed into pale yellow. Another small diamond, of so dark a green as to approach black, and almost opaque, assumed a violet hue, retaining, however, its brilliancy and becoming much more translucid. A small cubic diamond of light green colour preserved its brilliancy and transparency intact, but lost its colour completely: no difference in its weight before and after the operation could be perceived. Brown diamonds lose most of their colour when heated to whiteness in hydrogen; they generally assume a greyish tint, in all cases the shade is much lighter, and on examination with a lens they appear limpid, with black spots. Diamonds of a yellow tint, such as Cape diamonds almost invariably are, scarcely lose any portion of their natural colour.

Since the last Exhibition at Paris in 1867, opportunity has been afforded of examining a very remarkable diamond belonging to M. Coster. Although almost colourless, upon being heated out of contact with the air (in a magnesia bath) it assumed a deep rose colour, which it retained for some days when kept in the dark; when exposed to the light, however, particularly that of the sun, the colour rapidly disappeared, but could be restored by again heating it. On examining a rose-coloured diamond, expected by M. Coster to acquire a deeper tint upon exposure to heat, it was found, on the contrary, that the effect of the operation was to deprive it of colour, which it afterwards gradually regained. Several experiments were made by von Baumhauer, in concert with M. Daniels, upon grey diamonds, in the hope that the effect of heat would, by removing the colour, add to their value; but unfortunately the desired result was not achieved, as the diamonds presented after treatment the same greyish aspect as before.

Very different effects are obtained when, instead of heating the diamond in an atmosphere of hydrogen, it is heated in contact with the air. It is unnecessary to employ a white-heat, or to subject the diamond to it for so long a time, in order to render it dull, and consequently opaque; this being the result of positive combustion, which is proved by its loss of weight after the operation. This combustion is, however, quite superficial, as shown by M. Daniels, who found that when re-polished, the diamond recovered completely its transparency and its water; it was, moreover, remarked by M. G. Rose that if the diamond which had become dull was moistened with essence of turpentine, it reassumed its transparency, and retained it as long as its surface continued moist.

The diamond may also be heated in an atmosphere of oxygen, by introducing a current of that gas into the crucible through the small platinum tube before mentioned; in this case the stone attains a vivid state of incandescence, and burns with a dazzling flame long before the platinum crucible has attained a reddish-white heat. In most cases, after the lamp has been withdrawn and the crucible is no longer red-hot, the diamond continues to burn for some time, and presents an appearance of vivid light upon a dark ground. When the diamond is very small combustion may even continue until it is entirely consumed, and it is then seen to dart a more vivid flame at the last moment, like a burning match, the instant previous to extinction. When the stone is of considerable size, the heat produced by combustion is insufficient to maintain it after the removal of the lamp, and it ceases in a few moments notwithstanding the oxygen which continues to flow into the crucible.

Although this last experiment has been repeated several times by these experimentalists, no other result has been observed than tranquil combustion of the diamond; such phenomena as turning black, transformation into coke, change of the state of aggregation, bubbling up, melting or softening, rounding of corners and angles, were in no case presented to our notice. Once only in experimenting upon an opaque greyish diamond, a few sparks were emitted, but these were evidently due to the presence of some foreign elements incorporated with the whole. Neither did the diamonds burst or split, save in one case, where such was foreseen by M. Daniels: a stone, evidently composed of two diamonds joined together, upon the first application of heat broke with considerable violence into two fragments, each constituting a decided crystal.

It has been asked if the combustion of the diamond in oxygen or atmospheric air is accompanied by flame. M. G. Rose denies this completely, but his mode of operation, namely, by heating the diamond upon a cupel in the muffle of a reverberatory furnace, and drawing it out from time to time for examination, or by heating a thin piece of diamond upon platinum foil in the flame of a blowpipe, was not well calculated to settle the question. On the contrary, by the above method, all that took place in the crucible could be distinctly seen through the sheet of mica, and thus ample evidence was obtained that the diamond, while in a state of combustion is surrounded by a small flame, the exterior envelope of which is a violet-blue, similar to that produced by oxide of carbon in a state of combustion. This is especially the case when the diamond is rather large, when the lamp has been withdrawn and the platinum has ceased to glow: the diamond is then seen upon the black ground of the crucible, brilliant with vivid white light, and surrounded by a zone or aureole somewhat less bright, its exterior edge being a blue-violet colour.

Some highly interesting microscopic observations relative to the dull surface of diamonds which have undergone partial combustion have been communicated by M. G. Rose; he has discovered on them regular triangular markings that resemble those occurring in abundance on the fine crystals from the Vaal River, and recall the faces formed on planes of crystals, soluble in acid, by the slow and imperfect etching action of such a reagent; as, for example, the action of hydrogen chloride on calcite. Like them, these depressions on the diamond bear an exact relation to the crystalline form, and are determined by certain definite faces, their sides being parallel to the edges of the octahedral faces of the crystal. Measurement with the goniometer shows them to belong to the icositetrahedron, the faces of which have not been met with on diamond. These symmetrically shaped pits can easily be seen by heating a thin plate of boart in a blowpipe flame and examining it under the microscope. By prolonged heating several small triangular pits will often merge into one large one. A crystal of diamond, even when so reduced in size by oxidation as to be only visible with difficulty, continues to exhibit sharp edges and angles. A dodecahedron with very rounded faces but smooth and brilliant surface also exhibited the triangular pits often very distinctly; moreover, it had a brown colour, which was not destroyed by heat, and must, therefore, be of a totally different nature from that of the topaz or smoky quartz.

Several experimentalists, M. Jacquelain amongst others, affirm that at an extremely high temperature, such as is attainable at the focus of a large burning-glass, or between the charcoal points of a powerful galvanic battery, such as 100 elements of Bunsen, the diamond softens, that it passes into an allotropic state, is changed into true coke, capable of employment as an excellent conductor of electricity, and diminishes greatly in density, as much as from 3·336 to 2·6778. It has also been stated, that upon watching through smoked glass the combustion of a diamond under the focus of a burning glass, it was seen to melt, and even to undergo a kind of ebullition.

M. Schrötter informs us that the R. T. cabinet of mineralogy, at Vienna, contains a diamond which was placed under the focus of a burning glass in 1751, by Francis I., the husband of Maria Theresa, and allowed to burn for some time, and that after this partial combustion the diamond, a very limpid well-cut stone, became black both externally and internally.

Clarke, having burnt a diamond in the flame of oxy-hydrogen gas, relates that it first become opaque, like ivory, then the angles of the octahedron were rounded, the surface was covered with bubbles, and there remained a globe of metallic brilliancy, which finally disappeared entirely. Silliman, upon burning a diamond upon magnesia, found it turn black and burst, and Murray and Macquer also speak of the diamond turning black under combustion.

Messrs. Rose and Siemens heated the diamond between the two charcoal points of a large magneto-electric machine, the poles being enclosed in a glass cylinder from which air was excluded. During two separate experiments, upon the charcoal becoming incandescent, the diamond exploded into numerous fragments, all of which were black; examination showing, however, that the colouring was wholly superficial, and that the interior had undergone no alteration: the blackened fragments could be used for writing on paper. From these experiments, and also from the one described above in which the surface of the diamond had turned black after exposure in a crucible of charcoal to heat capable of melting wrought-iron, the conclusion drawn by M. Rose is, that under the influence of excessively high temperatures, the diamond, although preserving its shape, begins to change into graphite, and would probably do so entirely if the heat were sufficiently strong and prolonged for the requisite period.

Opportunity for a repetition of these experiments, not having occurred to M. von Baumhauer, he has not given an opinion upon the behaviour of diamonds at extremely high temperatures; it may, however, be remarked that the blackening which occurs when the diamond is placed in the voltaic arc, may result from transmission of carbonaceous particles from the charcoal poles to the surface of the diamond, which would retain them without the occurrence of any radical alteration. During the employment of the burning-glass, the support upon which the diamond was held might possibly contain matter, which on coming in contact with the carbon of the diamond at so high a temperatue might give rise to reductive phenomena conducive to the formation of the black coating. Something of the kind was observed by M. Schrötter, in an experiment in which the diamond was placed in a crucible in the centre of a mass of strongly compressed magnesia, and moreover folded in a thin sheet of platinum, and then exposed to excessive heat in a porcelain oven. After cooling, the diamond was found to be divested of its platinum cover, which had melted into a globule and adhered to one of its facets. The exterior of the diamond had turned black, whilst its interior was permeated with black dendritic striæ, giving rise to the supposition that a combination of carbon and platina had occurred.

Without having employed the extreme heat attainable in the arc of a powerful galvanic battery, or at the focus of a burning-glass of large dimensions, M. von Baumhauer has, nevertheless, more than once heated diamonds in the oxyhydrogen flame (that is to say, to a temperature capable of melting platinum *) in which the stone emitted a brilliant light, and lost weight rapidly; after the experiment the diamond of course appeared dull, but not the least appearance of blackening was observed, either on the surface or in the interior. Neither was it remarked by M. Jacquelain, when operating with a flame produced by a mixture of oxygen and hydrogen in proportions necessary to form water, or by one composed of a mixture of oxygen and oxide of carbon. The experiment was interrupted several times to examine the diamond, which nowhere presented either brown spots or blackening.

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* When the points of the platinum which held the diamond were touched by the flame, not only did they melt, but upon examination through smoked glass the platinum was seen to be in decided ebullition.

M. Jacquelain considers that perhaps the surfaces of the diamond have been blackened, and that this has disappeared again owing to contact with carbonic acid, and aqueous vapour at a high temperature; in fact, from the considerable friction resulting from the gaseous mixture escaping from a receiver under strong pressure. However that may be, this experiment proves incontestably that the flame resulting from a mixture of hydrogen and oxygen in the same proportion as in water, is incapable of softening the diamond, and that the combustion of this explosive mixture is far from producing the energetic effect of 100 elements of Bunsen. M. von Baumhauer considers that the transformation of the diamond into coke or graphite by means of heat is still to be doubted; nor should it be admitted, until it is quite certain that blackening is not the result of chemical action produced by foreign matter, or by the transmission of carbonaceous particles from the charcoal poles to the surface of the diamond.

To ascertain whether the diamond would be capable, at white heat, of decomposing water, and burning by means of the oxygen contained in it, there was passed over a rough limpid diamond, and also over a cut diamond, a current of superheated steam, in a platinum tube exposed to the heat of a flame of gas urged by a current of air. Although the operation was continued for ten minutes, the diamond was quite brilliant after cooling, and had lost nothing of its weight; proving, that at this temperature at least the diamond suffers no change in an atmosphere of superheated steam.

It is otherwise, however, when the diamond is kept for some time in an atmosphere of dry carbonic acid. A rough stone, weighing 0·1515 grm., was subjected to a white heat for ten minutes in a crucible closed with mica, supplied with dry carbonic acid already flowing, some time before the application of heat; when cooled, the surface of the diamond was dull and its weight decreased by 0·0015 grm. This experiment was repeated with a cut diamond weighing

0·6095 grm.: when withdrawn from the crucible it had become quite dull, with the exception of two facets which had preserved their brilliancy, but were tinged with iridescent colours; the carbonic acid current had exerted upon them a comparative cooling action; the stone had lost about 2 milligrammes. This proves that the diamond is capable at a white heat of decomposing carbonic acid, and of combining with its oxygen, but the action is very slow. This decomposition had already been perceived by M. Jacquelain, although his mode of operation was uncertain in his results. A receiver was employed with two openings, and filled with carbonic acid; one opening communicated with a tube, at whose extremity the oxyhydrogen gas was burnt; through the other was introduced the diamond supported on a piece of pipe-clay. In this experiment the diamond was consumed rapidly, but especially by the oxygen of the oxyhydrogen mixture, no trace of blackening being perceptible.

III. ON COMPARATIVE VEGETABLE CHROMATOLOGY.

By H. C. Sorby, F.R.S., &c.

IN an article on the various tints of autumnal foliage, published in this journal for January, 1871,[*] I described a number of the leading kinds of colouring matters found in the higher classes of plants. Since then I have studied them far more thoroughly; so that what I said must be looked upon as a mere commencement of a subject which has now become so extensive, and includes special modifications of so many branches of science, that it appears desirable to give some single name to the whole. It is for this reason that I have called it *chromatology*. In this former paper my chief object was to describe the cause of the production of the various tints of leaves when they fade. This is mainly the result of chemical changes taking place when the leaves are dying or dead, which correspond very closely with what can be imitated artificially by acting with various reagents on the dead materials extracted from the living plants. On the contrary, my object now is to describe the different coloured substances formed by the constructive energy of living plants, or changes that take place in them whilst they are still portions of living organisms, which changes can be imitated very imperfectly, and I shall only incidentally notice alterations which occur after the plants are dead.

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[*] New Series, vol. i., p. 64.

When first I commenced the study of the colouring matters, I was very well contented to confine my attention to those which occur in relatively large quantity in flowers and green leaves, or give striking and well-marked spectra. On extending my researches to fungi, lichens, and algæ, I soon found that the more abundant substances were very different in different classes of plants; and on making more careful comparisons, I found that some of the colouring matters which occur in a relatively large quantity in one class are often not really absent from others, but occur in relatively small amount. This led me to discover that the coloured solutions obtained from green leaves are even more complex than had been supposed, and that, independent of those soluble in water, they contain normally no less than six or seven coloured substances, perfectly well distinguished by their optical and chemical characters. Having determined the chief coloured constituents of the leading classes of plants, I drew up a rough table, showing their distribution through the great groups of the vegetable kingdom, and saw at once that there was such a striking connexion between the general organisation of plants and the character of the colouring matters contained in them, that it was desirable to explore the question as completely as possible. This inquiry would have been very difficult, if not impossible, if I had not been able to contrive fresh means for separating or otherwise recognising the different constituents of complex mixtures.

Many of the most important coloured substances met with in plants are insoluble in water, but soluble in bisulphide of carbon and in alcohol, but the relative facility with which they are dissolved by these two reagents differs very much. When dissolved in spirits of wine of the usual strength, and the solution agitated with excess of bisulphide of carbon, the whole of some of them is carried down in the bisulphide, whereas the whole of some other substances is left in the alcohol if it be strong, but more and more is carried down in the bisulphide when the alcohol is more and more diluted with water. The result is that some substances can be separated perfectly, and others only partially; but by agitating the solution in spirit with excess of bisulphide, separating the alcoholic solution, and repeating the process over and over again, with the addition of a little water each time, a comparison of the spectra of the different portions thus fractionally separated will often suffice to show whether the original coloured solution was or was not a mixture, and the extremes are sometimes different substances, in a more or less pure state. Of course if the original had not been a mixture, such a difference would not occur, unless some decomposition took place, which could easily be detected. There are, however, cases where different substances cannot be separated in a satisfactory manner by such means, and it would be almost impossible to study comparative vegetable chromatology successfully, if light could not be made use of as a reagent. On exposing to the direct rays of the sun solutions of different colouring matters in bisulphide of carbon or other solvent, some are rapidly decolourised, usually, but not always, without the intermediate production of any new coloured substance, whilst others fade very slowly, some being changed by one kind of light, and some by others. This decomposition usually depends upon the presence of both air and light, and does not occur in the dark when air is present, or in tubes quite free from air when exposed to the sun. The result of this difference in the behaviour of different substances is that, though they cannot in some cases be separated by chemical means, one may be entirely destroyed by exposure to the open sun, or to particular rays which pass through coloured glasses, whilst sufficient of the other remains unchanged to show its characteristic properties in a satisfactory manner. By combining the above-described method of fractional separation with this kind of photo-chemical analysis, it is often easy to unravel very complicated mixtures; and I do not think that anyone who had not tried this system of investigation would be prepared to find how much may be effected by such simple means. By adopting these several methods, it is not only possible to detect a comparatively small quantity of the more important constituents in complex mixtures, but also to determine their relative amount in different cases. This kind of comparative quantitative analysis is of very great value in the present subject. It does not consist in ascertaining the relative weight of the different colouring matters in any one specimen, like the ordinary sort of quantitative analysis, but in determining the relative quantity of each colouring matter in two or more different specimens. In those cases where the constituents can be more or less perfectly separated, the relative quantity of each kind can be easily determined by having the solutions in two tubes of equal diameter, and diluting one or both until the depth of colour is the same, or still better, until the spectra exactly correspond when compared side by side. The relative amount of each is then known by measuring the length of the columns of solution in the tubes. It is also often easy to ascertain the relative amount of more than one constituent in similar solutions; for the absorption-bands of one may first be made equal, and then those of another, measuring the relative volumes when the solution of each colouring matter is thus found to be of equal strength. For the future I intend to try to carry out this sort of analysis by means of standard solutions, sealed up in tubes free from air; and, if they remain permanent, the relative composition of mixed solutions could be determined without actually comparing them together, which would make it possible to ascertain the changes that take place in plants at different seasons of the year, and to otherwise develope the subject to a far greater extent than heretofore. I have also lately adopted another method, which makes it to some extent possible to dispense with such comparisons. I endeavour to determine the relative proportion of the various coloured constituents in terms of the amount of light absorbed by each. When this is in nearly the same part of the spectrum, the comparison can be made with considerable accuracy; but when it is in very different parts, only approximately, but yet in such a manner as to yield far better results than any other method.

In applying these principles, of course there are many questions of detail, depending on particular circumstances; and what I have now described must be looked upon merely as such a general account of the methods I have adopted as seemed to me desirable, since otherwise the possibility of determining some of the facts might have appeared doubtful. I now, therefore, proceed to the consideration of the subject more immediately claiming attention.

Comparative vegetable chromatology may be divided into two principal parts, viz., that in which we compare leaves or fronds of the same kind of plant growing in different conditions, in order to learn the effects due to external influences, and that in which we compare different plants growing in similar conditions, in order to learn the effects due to internal organisation. Some of the effects of a difference in the amount of light are well known. When it is almost absent, the leaves are yellow and pale, owing to chlorophyll and some other colouring matters not being properly developed; but I have found by careful comparative quantitative analysis that, when plants are exposed to more light than is requisite for their healthy growth, the amount of chlorophyll and other colouring matters is diminished sometimes to even one-third of the maximum quantity. I have also found that, when a leaf is partially covered up and screened from the light, the amount of chlorophyll increases in the shaded part. In the case of a leaf of *Aucuba japonica*, chosen for the experiment because it is much influenced by light, the increase was no less than at the rate of two per cent per diem. Chlorophyll separated from the leaves is rapidly decomposed by light, and it could scarcely be supposed that a similar change would not to some extent occur in the living plants. In fact the power with which it then resists such a change seems to require special explanation. The general connexion of all the facts I have observed leads me to conclude that some, if not all, the coloured constituents of growing leaves, like the constituents of the bodies of animals, are in a constant state of transformation, new being formed and the old destroyed, the apparently uniform composition being due simply to the establishment of an equilibrium, which remains nearly the same when the conditions are the same, but is very soon changed when they are altered. This supposition explains in a satisfactory manner many facts which would otherwise be unintelligible, and probably one result is that the endochrome* is thus constantly maintained in a young and vigorous condition. According to this view of the subject we may suppose that, in the above-named case, when the amount of chlorophyll apparently increased at the rate of two per cent per diem, the relative increase was due, not to more being developed when the light was excluded, but to more being decomposed in a corresponding portion of leaf left exposed to the sun. The equilibrium of the constituents was thus partially changed from that found in leaves when growing much exposed to the sun to that of leaves growing in the shade.

On comparing the relative amount of the other constituents of various plants, when more or less exposed to the sun, I have found that equal weights of the leaves or fronds contain almost the same amount of those colouring matters which are the least changed by the action of light, and that the relative quantity of the others in those leaves exposed to the sun, decreases in the same order as they are more and more rapidly decomposed by the action of light, and in proportion as the leaves or fronds are exposed to more and more light. There is thus established a sort of equilibrium, varying with these different conditions, and easily explained, if we suppose that the different coloured substances are being constantly formed by the internal constructive energy of the plant, and constantly decomposed in varying proportion by the destructive action of the oxygen of the atmosphere, intensified by the influence of light. There are, however, well-marked exceptions to this rule, which require us to suppose that the constructive force varies qualitatively as well as quantitatively, when it is much reduced by the absence of light or other causes, so that some of the different compounds are formed in very different proportions. The development of fructification also sometimes produces a certain amount of alteration, as though the colouring matter formed in the organs of reproduction were abstracted from the fronds. In the case of the lichen *Peltigera canina*, when it grows in a very damp and shady situation, there is a greater relative deficiency of certain colouring matters, which I have named *lichnoxanthine* and *orange lichnoxanthine*, than seems likely to be due to the decomposing action of light on the other constituents of the specimens more exposed to the sun, and the relative amount is again decreased by much more exposure. On the whole it appears more probable that the deficiency is mainly due to their imperfect development when there is too little or too much light for the healthy growth of the plant, and this fact is of much interest, when we know they are the characteristic colouring matters of the fructification, and that it is imperfectly, or not at all, developed in very exposed or in very shady situations, perhaps because these requisite substances are not formed in sufficient quantity. I have also found that there is a most remarkable alteration in the relative amount of the different coloured substances characteristic of *Oscillatoriæ*, when the light is very feeble, evidently due to the weak constructive energy, as will be more fully considered in the sequel.

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* I think it would be found very convenient to adopt this term as the name for all kinds of simple or complex coloured substances found in the cells of plants.

Having thus learned what is the character and the extent of the changes produced by varying conditions on the colouring matters found in the same, or in closely allied, species of plants, we are in a better position to understand the variations corresponding to the difference in the general organisation of different classes, and to distinguish and eliminate the effects due to special conditions. The facts described above clearly show that, if we wish to ascertain what changes depend on a difference in organisation, it is necessary to compare normal specimens of each class, growing as nearly as possible in similar circumstances; though, at the same time, it is very desirable to determine what is the effect of different conditions.

In studying comparative vegetable chromatology, it is requisite to distinguish between fundamental and accidental colouring matters. There is the same sort of difference in the case of animals. The hæmoglobin of the blood, and the colouring matters in the bile are, as is well known, of such great physiological importance, that they are essential to the healthy life of the higher classes of animals, whereas the colouring matters in the hair or feathers are of only very indirect utility. In a similar manner the higher classes of plants cannot permanently grow without the colouring matters belonging to the chlorophyll and xanthophyll groups, whilst the various red and blue substances belonging to the erythrophyll group may be present or absent without materially interfering with the growth of the plant, and are either of no use, or only very indirectly advantageous, as for example, in attracting to the flowers the insects instrumental in causing fertilisation. At present it is impossible to decide whether certain kinds of colouring matters are or are not essential to the growth of particular plants, or whether they may not be necessary for some classes, and present in others like those organs of animals which, though requisite for some classes, are only rudimentary and of no use to others. Some, also, may be only constant products. The whole subject is, indeed, only in its infancy; many fundamental questions remain to be decided, and for the present we must be content with having obtained a clue to a kind of research which promises to throw a new light on such inquiries.

It is very common to find that accidental colouring matters are much more conspicuous than some that are probably of great importance. Thus, for example, the crimson-coloured substance which is developed in the leaves of certain varieties of the beech, is so very conspicuous, and disguises the other colouring matters so much, that perhaps few persons would imagine that the normal amount of chlorophyll is present, and yet this is easily proved by comparing the spectrum of a very red leaf, growing where much exposed to the sun, with that of a green leaf, growing in a very shady place on the same tree, the absorption-band of the chlorophyll being almost equal. This red colouring matter is probably a product of the decomposition of chlorophyll, due to the action of light, when the leaves are in a peculiar low state of vitality. The result of such a predominating influence of what may be called accidental substances is that mere *colouring* is often of very little general significance, even in distinguishing closely allied species. This is, however, quite intelligible, since comparatively small special differences in the constitution of the individual plants may suffice to alter the character of the accidental colouring matters, especially in organs like the petals, which are not essential for the life of the individual plant. In fact, by artificially lowering the constructive energy, by screening flowers from the light, I have succeeded in producing as much change as would have corresponded to well-marked varieties, if both had been exposed to the light. When, however, careful qualitative and comparative quantitative analysis are compared, which appears to me to be the only correct way of studying the subject, it becomes quite apparent that there is a very interesting connection between the distribution of the fundamental colouring matters and the general organisation of plants. In proceeding from the lowest to the highest classes there is an unmistakable advancement from a type corresponding in certain particulars with that of some of the lowest animals to that of the highest classes of plants, as though certain colouring matters were more characteristic of, and perhaps indeed essential for, the healthy growth of the most perfect and specialised types of vegetable life. There are also remarkable examples of the changes in the colouring of particular plants, according as they grow in strong light or in such very shady situations that the vitality is very low, and on comparing the qualitative and quantitative differences it may be seen that in several important particulars they correspond with the differences met with in higher or lower classes, the effect of the comparative absence of light being to lower, and the effect of the presence of extra light being to raise the type. The most striking instance of this so far met with is in the case of *Oscillatoriæ;* for when they grow where the light is so feeble that they can only just keep alive, the type of their colouring approximates to that of olive *Algæ*, whereas, when they grow exposed to much air and light, the type approximates very closely to that of such lichens as *Peltigera canina*.

In order to show the kind of evidence on which such conclusions are based, and also to illustrate what I mean by comparative quantitative analyses, I subjoin the following table. It would have been of very little use to have compared equal weights of different plants, since the amount of endochrome is so very different, and it is the variation in its composition that is of chief interest. It was therefore necessary to assume some one constituent equal in all cases, and the only one suitable for this purpose was the blue chlorophyll. This, then, was taken at 100 in all the specimens, and the relative amount of the other constituents calculated accordingly, 100 being taken as the maximum quantity for each kind of colouring matter. By adopting this plan, of course the amount of some of the constituents is made to appear as though it were increased by greater exposure to light, in the same order as they are less and less decomposed by its action; but in reality the amount is diminished in the opposite order, as would be made apparent by assuming as equal the constituent least changed by light, or by taking an equal weight of the different specimens of each separate kind of plant.

	Blue Chlorophyll.	Chlorofucine.	Phyco-xanthine.	Orange Xanthophyll.	Fucoxanthine.	Lichno-xanthine.
Fucus grown in the shade	100	90	0	3	77	11
,, ,, sun	100	100	0	3½	100	14
Oscillatoriæ under water in a very shady place	100	13	0	1	51	6
Oscillatoriæ in more light	100	19	36	3	55	10
Oscillatoriæ in and on water, exposed to the sun	100	trace	67	25	11	9
Oscillatoriæ on a damp wall quite open to the sun and air..	100	trace	100	77	25	23
Peltigera in a medium amount of light.. ..	100	trace	27	32	0	32
Peltigera exposed to much sun	100	trace	54	100	0	100

On comparing the relative quantities of the different substances, it will be seen that, as before named, very great changes are due to the difference in the amount of light, some of which may be referred to its direct decomposing action, and others to its indirect influence on the constructive energy of the plant. The most important general fact, however, is that, when the *Oscillatoriæ* grow in a very feeble light, the phycoxanthine and orange xanthophyll almost or quite disappear, whilst the amount of fucoxanthine increases, so that the general relation of the colouring matters approaches that of the *Fucus;* whereas, when they grow in bright light, the amount of fucoxanthine is much decreased, and that of the lichnoxanthine considerably increased, whilst the phycoxanthine and orange xanthophyll are developed to a remarkable extent, so that the general type approaches that of such lichens as *Peltigera*.

There is also a well-marked tendency to approximate to a lower type of colouring in the case of those permanent varieties of plants which have very yellow leaves, on account of the amount of chlorophyll being abnormally small. The green leaves of the higher classes of plants contain two different kinds of chlorophyll, which give quite different spectra, and differ in various other important particulars. These I have named *blue chlorophyll* and *yellow chlorophyll*, from the difference in their general colour. Now the small quantity of chlorophyll which exists in the above-named leaves contains only about one-third the relative amount of the yellow chlorophyll, which corresponds to what is met with in leaves abnormally yellow from being grown almost in the dark, as if both were due to low constructive energy, one natural to the variety, and the other produced artificially. Both differ greatly from green leaves which have turned yellow by fading, for these contain double the normal relative amount of yellow chlorophyll, which is not so readily formed, but, when it has been formed, is not so readily decomposed as blue chorophyll. This reduction in the relative quantity of yellow chlorophyll causes leaves abnormally yellow, owing to low constructive energy, to approach to the type of red *Algæ*, in which this energy is so low that blue chlorophyll is developed alone, and yellow chlorophyll is quite absent. If further research should prove the existence of other examples of this kind of fact, and establish it as a general law that when the healthy development of the higher classes of plants is arrested the type of colouring approaches to that of lower classes, it will be very instructive in connection with the theory of evolution, and analogous to what is so common in the general structure of animals, in which when their development is arrested it often approximates more or less to that of those of lower organisation. It would also indicate that in some way or other the constructive energy of the lowest classes of plants is lower in the scale than that of the highest, but it does not follow that plants with this higher type of constructive energy could live in more variable and adverse conditions than those with a lower type.

It would be impossible to select a better example of the manner in which different groups of plants are related, and also distinguished by their colouring matters, than the leading divisions of marine *Algæ*, viz., the olive, the red, and the green. These very seldom contain accidental colouring matters, and the result is that the general colour is such a good indication of the kind and relative proportion of the fundamental colouring matters that it has been generally recognised as a valuable means for arranging *Algæ* into the above-named three divisions, though the true relations and differences were unknown. The total number of the fundamental substances is about twelve, and for a description of their distinguishing characters I refer to my paper, recently published in the "Proceedings of the Royal Society" (vol. xxi., p. 442).

Now these various substances are distributed very differently through the different groups, so as to connect and yet distinguish them in a very definite manner. I have not been able to make accurate quantitative analyses, and, besides that, there is a considerable variation in the relative quantity of some of the constituents of the red group, and therefore it is only possible to give a general tabular view, by expressing the relative amount of the various substances by means of the following signs:—

A relatively large quantity . . . . ✻
" " moderate " . . . . +
" " small " . . . . •

	Olive Group.	Red Group.	Green Group.
Blue chlorophyll	+	+	✻
Yellow chlorophyll			+
Chlorofucine	+	•	
Orange xanthophyll	+	+	+
Xanthophyll		+	+
Yellow xanthophyll			+
Fucoxanthine	✻	•	
Lichnoxanthines		•	+
Phycocyan		+	
Pink phycocyan		✻	
Red phycoerythrine		✻	

On inspecting this table it will be perceived that the olive *Algæ* are characterised by the large amount of chlorofucine and fucoxanthine, and by the total absence of yellow chlorophyll, of xanthophyll, and of yellow xanthophyll. The red *Algæ* are especially distinguished by the colouring-matters of the phycocyan and phycoerythrine groups, but also differ from the olive in containing xanthophyll and only a little chlorofucine and fucoxanthine. The green *Algæ* are characterised by the presence of yellow chlorophyll and yellow xanthophyll, as well as by the absence of chlorofucine, fucoxanthine, and the substances specially characteristic of the red group. Blue chlorophyll and orange xanthophyll are common to all, and it is probable that no class of plants except fungi is ever quite free from both of them. It will also be seen that the red group is intermediate between the olive and the green, and independent of the colouring matters specially characteristic of the red, it differs from each of the other groups far less than they do from one another, and, besides this, there are still closer connecting links, not shown in the table.

My endeavour has been to extend such a method of comparison to all the leading classes of plants and some of the lower classes of animals, and to ascertain the order in which they should be arranged, so as in like manner to have the most gradual and unbroken passage from one to the other. Comparing these various groups of *Algæ* with other classes of plants, and with such low classes of animals as *Actiniæ*, I found that the whole of the colouring matters present in the green *Algæ* are those most characteristic of all the higher plants, the only difference being that in certain circumstances these latter contain in addition various more or less accidental substances belonging to the erythrophyll and chrysotannin groups, which to some extent appear to be characteristic of particular classes. As far as colouring is concerned, the green *Algæ* are therefore perfectly typical plants. On the contrary, the olive *Algæ* differ from them in a very marked manner. They contain no yellow chlorophyll, or either of the two kinds of xanthophyll, all three so very characteristic of the most perfect plants, but contain chlorofucine and fucoxanthine, both of which are found in certain species of *Actiniæ*, like *Anthea cereus*, var. *smaragdina*. The presence of such colouring matters, therefore, connects them with some of the lowest classes of animals, in the same manner as the presence of chlorophyll connects such animals with plants.

I have extended this method of comparison to many other cases, but much remains to be learned before the exact connexion of all the leading groups of plants can be looked upon as established in a satisfactory manner, and I have hitherto been unable to obtain suitable material for thoroughly investigating the relation between the lowest classes of plants and animals. Though I look upon my present results only as a beginning of the subject, it may, perhaps, be well to explain what is the general bearing of the facts so far determined, and to give a tabular view of the manner in which some of the different classes of plants should be arranged, so as to be in the order of the most simple continuity. This table, of course, refers only to the chromatological characters, and since we could scarcely expect them to follow the same order as the structure, we cannot be surprised to find that the order of arrangement is not exactly the same as that so commonly adopted, and yet the general agreement is sufficient to show that a similar great principle is common to both.

Actiniæ.
Anthea cereus, var. *smaragdina.*
Olive group of Algæ.

Red Algæ.	*Oscillatoriæ.*
Porphyra.	*Peltigera.*
Green Algæ.	*Lichens.*

Higher Cryptogamia.
Highest classes of plants.

It was some time before I could understand how fungi should be placed in this arrangement, for they could not be inserted anywhere in the direct series. At length I found that on the whole their most prevalent colouring matters correspond with those characteristic of the fructification of lichens, and that fungi, therefore, bear much the same relation to lichens that the flowers of a leafless parasitic plant bear to the foliage of the highest class of plants. This conclusion, derived from the study of the colouring matters, agrees so well with what has been deduced from other quite independent data, connected entirely with structure, that it must be looked upon as additional evidence of an important relation between the general organisation of plants and their coloured constituents; which unites with other facts in showing that they are not mere chemical products, formed under such conditions as can be imitated artificially, but in some way or other depend on structure, or on forces connected with it in living organisms.

My attention has lately been directed to the study of the changes which occur during the growth of the various organs of plants. For example, I have compared the constitution of the endochrome of the petals, when in a rudimentary state, with that of leaves and fully developed flowers. Even so far the results are of much interest. The endochrome of the rudimentary petals approximates in character to that of leaves; and, during their development, this leaf-like character is gradually lost, and often new colouring matters are formed. Differently coloured varieties are often simply cases in which this development is arrested, so that some, when fully grown, correspond to others in a more rudimentary state; and if the development be arrested by unfavourable conditions, artificially produced, this rudimentary character of colouring is retained when the petals are fully grown. One of the most remarkable facts is, that in some cases, if we slowly oxidise the mixed colouring matters dissolved out from a flower grown in the light, by adding a little turpentine, or by exposing the solution to the sun, the relative proportion of the different substances is changed, so as to closely correspond to that met with in the same kind of flower grown nearly in the dark. Exposure to light thus produces the same effect on the dead colouring matters that absence of light produces in the living plant, which seems to show, that, when the constructive energy is weak, those substances which are most easily decomposed are not sufficiently protected from decomposition. The study of such changes during the growth of other parts of plants cannot, I think, fail to throw much light on several interesting questions.

Such, then, is a brief account of some of the leading features of what appears to me to be a very promising branch of research. There are many questions connected with it that I have alluded to in the most incidental manner or not mentioned at all. The study of the action of light on the various coloured substances when in different conditions, and dissolved in different liquids, either when alone or mixed with others, is of itself a wide field for inquiry, well worthy of attention, since it may serve to explain the manner in which the energy of the sun's rays becomes stored up in the various compounds formed by plants. I have studied this action very carefully, and though I have been able to detect what appear to be general laws of much interest in connection with optics and chemistry, very much still remains to be learned. The chemical relations of the various colouring matters require much further investigation, in order to ascertain whether and in what circumstances one may be artificially or naturally changed into another, which is especially interesting in connexion with the colour of the petals of flowers. It is also very desirable that the connexion between the decomposition of carbonic acid and the changes that take place in the colouring matters when exposed to the sun, should be more fully examined, since, when separated from the plants, their decomposition by light seems to be a process of oxidisation, which, of course, is the reverse of what occurs when living plants absorb carbonic acid and give off oxygen. Perhaps it is only those portions of the endochrome which in some way or other have lost their normal power that are thus destroyed, so as to make way for the younger and more active. It will also be requisite to still further study the variations in the spectra of the different colouring matters which depend upon the conditions in which they occur, since in some cases it is thus possible to ascertain whether they naturally exist in a free state or are combined with oily or waxy substances. This makes such a remarkable difference in the spectra of some yellow flowers, that for a long time I thought that the colouring matters were entirely different, but I have now found that when oil penetrates to the endochrome, so as to combine with the colouring matter, the spectrum is changed to exactly the same as that met with in other cases which are not thus changed by the addition of oil, as though sufficient had been naturally present. In these experiments, the petals must be well crushed, so as to burst open the cells, and then dried, or else the oil will not penetrate to the endochrome. By carefully examining the position of the absorption-bands, we may not only determine such facts as these in the case of a colouring matter insoluble in water, but when they are soluble we may sometimes prove that they are not dissolved in the aqueous juices in the living plant, but do become dissolved when decomposition takes place, as though perhaps originally enclosed in minute cells, and set free when the cell-walls decay. Independent of these various general questions, the study of all the leading classes of plants, and of a number of the lower classes of animals, is necessarily a very extensive subject that can only be worked out by degrees, on account of the difficulty of procuring the requisite materials, in a fresh state, at the proper season of the year; and it is made still more extensive by the necessity of examining the same plants at different periods of their growth, and when grown under different natural and artificial conditions. On the whole, then, it will be seen that comparative vegetable chromatology, in its full extent, including everything requisite for its successful investigation, is a very wide and almost new branch of science; and, though I have accumulated a large amount of facts, I cannot but feel that it is only in its infancy. Still, however, the brief account of it which I have now given will, I trust, suffice to show that even already it has thrown a new light on a number of facts, and that a further and more complete study will probably enable us to examine some of the most important questions connected with vegetable physiology and the evolution of plants, from a new and independent point of view.

IV. PEAT.

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As all questions connected with fuel are, at the present day, of primary importance, it is proposed, in the present article, to investigate the subject of peat, and the treatment it undergoes in order to fit it for all the different purposes, domestic and industrial, to which it has been, and is now, in some part, applied. Before, however, dealing with the various means by which it is prepared, it will be advisable to give some few details of its nature and peculiarities, as well as such scientific and statistical accounts of its available quantity and commercial value, as may tend to demonstrate more fully the vast mine of wealth lying hidden beneath the surface of bogs, and which is too often half neglected even where it is not wholly ignored and unutilised.

Notwithstanding the inferior calorific value of peat, which we shall further allude to presently, it has several undoubted advantages over coal, of which we may specially notice that it is more easily worked, and that without the necessity of any large expenditure on plant and machinery, such as is required for coal mines; the working of a peat bog is unattended with those dangers to life and limb which are so characteristic of coal mines; peat is reproductive, and can be cultivated, whereas the supply of coal existing in any country is incapable of being maintained, and must in course of time become exhausted. Peat can consequently be worked more cheaply and economically than coal. On the other hand, as we have already stated, the calorific value of peat is inferior to that of coal; its specific gravity being lighter, it is also more bulky, and consequently more costly, bulk for bulk, in carriage. Peat produces several valuable products on distillation, to which we shall refer more particularly as we proceed; it also produces a very pure charcoal which is highly esteemed in many manufactures. The best mode of carbonising peat for charcoal has been the subject of numerous experiments, but space will not admit of our entering upon this part of the question in the present article.

It will thus be seen that, although peat is undoubtedly inferior to coal as a fuel, it yet possesses many valuable properties and advantages which render it well suited as a substitute for coal, especially in localities in the near proximity to peat bogs, and where the carriage of that fuel would, therefore, not bear too high a proportion to its economic value.

According to Berthier and Regnault, peat gives from 28 to 30 per cent in weight of coke; lignite, 40 to 50 per cent; bituminous coal, 60 to 80 per cent; anthracite, 80 to 90 per cent; and graphite, 92 to 94 per cent of its weight. From a view of this table, it appears not at all improbable that we have in the above-mentioned substances a regular gradation of the action of nature in the production of pure carbon from vegetable substances. If we go a little further back, and take vegetable matter in the form of wood, we find that the amount of charcoal obtained from different kinds of wood varies from 16·4 per cent from Scottish pine to 26·0 per cent from lignum vitæ; the proportion of charcoal from the latter being therefore nearly equal to that from peat.

The following table of calorific values of combustibles, compiled chiefly upon the authority of experiments made by M. Péclet, is taken from M. Bosc's valuable work, "Traite Complet de la Tourbe," a book which affords more valuable information on the subject of peat generally than any book hitherto published:—

COMBUSTIBLES.	Calorific value of one kilogramme.	
	Dried.	Containing 25 per cent of water.
	Calorific units.	Calorific units.
Wood dried at 100°	3600	2750 With 10 per cent of water.
Anthracite	8000	7150
Pure carbon	7800	7000
Wood charcoal	7300	6500
Coke from moulded peat	7400	6500
Coal (first quality)	6000	5350
Ditto (second quality)	5500	4850
Coke from coal	6500	5800
Ditto from ordinary peat	5500	4900
Purified moulded peat	4500	3900
Ordinary peat	3200	2800

Experiments undertaken by the Chemin de Fer de l'Est, with stationary engines and locomotives, at their depôt at Epinal, both with coal and with peat obtained from the manufactory of M. Laroche and Co., at Sautaures, in the Vosges, gave the following results:—

Stationary Engine.—Peat consumed as fuel from the 1st to the 15th of August, 4500 kilogrammes (about 10 cubic metres). The machine was kept running for six hours per diem, consuming 50 kilogrammes per hour. With coal fuel the same engine running six hours per diem consumed 30 kilogrammes per hour. The peat gave a lively and clear flame, equal to that of coal, and it emitted no bad smell; the 4500 kilogrammes gave 7 kilogrammes of cinder and very little ash.

Locomotive Engine.—The same peat as fuel in locomotives, from the 15th to the 30th November, was tested to the amount of 4500 kilogrammes, or 10 cubic metres. Engines in reserve consumed 30 kilogrammes per hour while standing still, and 20 kilogrammes per kilometre when drawing goods trains. Similar engines burning coal fuel consumed 20 kilogrammes per hour, and 15 kilogrammes per kilometre, under similar circumstances respectively.

From these experiments it would appear that peat fuel is equal to two-thirds of the value of coal, and this is the standard of value which is most generally given to it; but much necessarily depends upon the purity of the peat itself, and upon the method in which it has been dried and prepared.

On the Bavarian State railways, it is stated that one cubic foot of ordinary air-dried turf, of rather a light description, and weighing from 16 to 18 lbs., as used there in the locomotive, is considered to be equal in heating power to about 13 lbs. of compressed turf, or 13 lbs. of lignite, or 7½ lbs. of coal.

Last year, owing to the exertions of Mr. Alderman Purdon, of Dublin, a Commission was organised "for the purpose of investigating, in the public interests, by personal examination in the fullest manner, such of the best modern systems of preparing improved fuel from peat as are now to be found elsewhere." The report of this Commission was presented in January last, in which, with reference to the economic value of peat as compared with coal, they remark very justly that "various estimates have been put forward of the relative heating powers of coal and turf—their comparative values as fuel—some rating peat as less than half the value of coal (weight for weight), others at two-thirds. These estimates may, in different cases, be true, for the value of peat is very variable, depending on its quality, density, and dryness; and in the combination of these, no two samples may be found identical."

Peat, as it is cut from the bog, contains from 70 to 90 per cent of water. Air-dried turf usually contains from 18 to

25 per cent of water. It does not appear to be capable of being dried in air beyond a point at which it will continue to retain about 15 per cent of water; and even when dried in a stove, it is never reduced beneath 7 or 8 per cent.

According to Dr. Ure, the calorific power of dry turf is only about half that of coal. This power is, however, immensely diminished in ordinary use by the water which is allowed to remain in its texture, and which the spongy character of its mass renders it very difficult to get rid of. Again, we find it stated, in "Tomlinson's Cyclopædia," that 7 lbs. of properly dried peat will evaporate the same quantity of water as 6 lbs. of Newcastle coal. No reliance can, however, be placed on any of these statements, so far as they concern the actual value of peat as a fuel, without more detailed information relative to the analysis of the peat used, the mode in which it was prepared, and the analysis of the coal against which it was tried. In the absence of this information, all reports on the relative values of peat and coal are unreliable, except as regards the experiments to which they relate, and worse than worthless for general purposes of comparison, as they are calculated to mislead rather than to give substantial facts that will apply in other cases and in other localities than where such results had been obtained. The fact, which is too often lost sight of by the advocates for the use of peat, is that that fuel varies in its nature and properties to a far greater extent even than coal. In speaking of the value of peat, therefore, care should be taken to define the quality of the substance referred to, in some such a manner as we now refer to coal of different kinds under the names of "anthracite," "steam," "bituminous," "cannel," &c., &c.; and until this is done, the statistics relative to peat and its performances must possess but a very questionable value. In order to show more clearly what we mean by this observation, we shall give presently some further particulars regarding the analyses of different peats, and the properties varying with its increased age, and its relative position in the peat bog. We shall thus see that great discernment is necessary for the judicious and economical working of a peat bog, as well as in its subsequent preparation as fuel.

According to Dr. Percy, the specific gravity of peat varies from 0·25 to from 0·6 to 0·9, and it varies in its contents of carbon from 32·28 in peat from Cashmere to 61·04 in peat from Kilbeggan, in Ireland. The value of peat as fuel will, of course, as a rule, vary according to the amount of carbon it contains, the peat having the highest proportion of carbon possessing also the greatest calorific value. The use of machinery and other appliances in the preparation of peat is chiefly to increase its density and to expel as much as possible of the water which it naturally contains. Now Sir R. Kane has stated that it is very usual to find the peat of commerce containing one-fourth of its weight of water, and that when dried in the air it will contain one-tenth of its weight. Hence the necessity for resorting to artificial means for getting rid of the water, which only has the effect of detracting from the value of the fuel; and it will be our object to point out some of the numerous devices which have been proposed for this purpose, and of the results obtained from them.

Almost all authors who have written on the subject of peat—with the single exception, so far as we are aware, of Dr. Zimmermann—assert that peat is a recent formation; for there appear never to have been found in it any remains of antediluvian animals, whilst bones of the ox, and the horse, horns of the stag and of the roebuck, and tusks of the boar are not of uncommon occurrence. The nature of the soil upon which peat bogs rest is that of ordinary vegetable earth, such as any other vegetation might grow upon. Frant and Sthel accord to peat a mineral origin; but there can, we think, at the present day, be few, if any, who would agree with them in this respect. Dejan says that peat is produced by marsh reeds and other aquatic plants, the stalks of which multiply, cross one another, interlacing themselves, and thus end by forming a solid mass of vegetable fibre. Other authors attribute the formation of peat to forests which, by reason of some natural phenomenon, have become thrown down and submerged, and, by a partial decomposition of wood and leaves, have given birth to peat. Zimmermann, again, gives another account of the growth of peat, which, he says, is formed of the decomposed roots of a group of plants, called *Sphagnum*. Peat of ancient formation, however, that author considers to be principally composed of the foliage and stalks of a reed-grass, or rush, of the roots of various aquatic plants, and of some peat-turf.

That peat is of vegetable origin there can be no reasonable doubt, but it appears in many varieties, according principally to its age and the circumstances of its origin. The foregoing remarks relative to the nature and character of peat, may at first sight appear to be not relevant to the subject more especially under consideration, namely, the different methods of preparing peat for fuel and the machinery employed therein. A little consideration will, however, prove the contrary; for it must be admitted that a correct knowledge of the nature of the material to be operated upon must necessarily precede any attempt to define the method by which it may be most advantageously prepared for its destined use. Its economical development also necessitates the consideration of questions affecting the working of peat bogs, as well as the possibility of their cultivation and extension.

Generally speaking, peat may be divided into two classes: that which contains nothing but terrestrial vegetable matter is called "bog peat," whilst that in which is found marine vegetable matter is known as "marine peat." Again, some authors recognise three classes of peat, which they distinguish as follows:—1. Bog peat; 2. Open country peat; and 3. Mountain peat. Sometimes two of these will be found in the same bog, merging, by insensible degrees, from the one to the other.

I. Bog peat is formed at the mouths of great rivers and of streams, as well as on their banks, on lakes, on ponds, and on the sea shore. The indispensable conditions for the formation of this kind of peat are a bed of water, rather shallow, and with only a moderate current, and the presence of certain vegetable matters, of a perennial growth, which, dying down each year, deposit at their roots the decaying vegetable matter, which, by constant accumulation, produces what is known as peat.

II. Messrs. Rennie, Dr. Walker, and Ch. Patin, besides others, consider that a great number of peat bogs owe their origin to a sudden destruction of forests; and this theory is strengthened by the fact that in many open country peat deposits there are found the trunks of trees embedded in the moss.

III. The mountain peat is formed chiefly by an innumerable quantity of mosses, of the genus *Sphagnum*. These mosses hold water in their stems like a sponge. They spread their roots and suckers over the moist *débris* of wood or other similar matters, and matting themselves together as they grow, form a species of felt, which, in course of time, yields the matter known as peat. One species of this plant, the *Sphagnum cuspidatum*, is so prolific that a single pod is said to contain no less than 2,800,000 seeds.

The cultivation and reproduction of peat is a subject regarding which there is now no longer any doubt. Experience of living witnesses has proved that it takes from thirty to forty years for a peat-bed to grow to the extent of one metre in depth, or about three metres in depth of good peat for fuel is produced in the course of a century.

It must be borne in mind that it is not possible to work all peat bogs with profit, and they are therefore again subdivided into those that are workable and unworkable bogs. When a bog is of a sufficient extent, and its peat of a good quality, not only should it be worked, but it should also be cultivated, the same principles of reproduction being applicable to peat bogs as to scientifically worked forests, where a system of clearing and planting are going on continually. One advantage of peat crops is that they have not to be gathered year by year, but the longer they are left, the greater and better in quality is their yield, and they are also independent of those variations in meteorological conditions which too often lead to the deterioration of other crops. After a careful consideration of those plants which are most productive in the formation of peat, it is necessary, for the sake of obtaining peat of good quality and purity, to protect the bog from the introduction of foreign matters. As these bogs are mostly situated in valleys, surrounded, for a part, at least, of their circumference, by hills, care must be taken to prevent silt being carried over their surface by heavy rains; the best way to accomplish this being to cut a trench round that portion of the bog exposed to such incursions. Unworkable bogs are those which do not produce peat of a sufficiently good quality to make it worth while to work them for fuel; these should be drained and cultivated.

Having now briefly treated of the nature and properties of growing peat, it may be interesting, before passing on to a description of the various methods of preparing it, to give some idea of the extent to which it is known to exist in different countries, in order to show how vast an area is available for the production and growth of fuel.

In France alone the peat deposits cover an area of about 1,200,000 hectares. They are spread over 58 departments, and are found in 5140 different localities. It does not appear that there are any peat bogs in Algeria; and, indeed, it is rather the exception than the rule to find this fuel growing in tropical climates. It has been stated by some authors that a temperature below 4° centigrade is necessary for the formation of peat, but this does not appear to be fully borne out by actual experience; for, as we shall presently show, peat deposits are to be found almost all over the world, but they are unquestionably most numerous in the more humid and temperate climates. In Belgium, the principal peat bogs are to be found in the environs of Ath and Antwerp, on the banks of the Escaut and of its tributaries, and in La Campine Belge. Peat deposits form almost the entire soil of Holland. The site of the Haarlem lake, upon which, in 1573, the Spaniards and Dutch engaged in naval combat, and which is now drained, and covered with luxuriant farms, consists entirely of a reclaimed peat bog. In Italy, one of the most remarkable bogs is that of St. Martin-Perosa, in Sardinia, which has an area of from 4000 to 5000 hectares, and a depth of from 4 to 8 metres. Piedmont also furnishes peat in some provinces; and it is stated that many of the paper and cardboard manufacturers about Turin employ 80, and sometimes as much as 90 per cent of peat in the pulp of their cardboard. Peat is also very generally found in most of the districts of Lombardy. The peat area of Denmark is estimated at about 180,000 hectares. In England but little attention has been given to peat, but the bogs in Ireland are said to cover about one-sixth of its entire area. Bogs of large extent also exist in Germany, Prussia, Russia, America, and Canada. Mouhot,* in his travels in Indo-China, &c., mentions the discovery of a peat bog in the Ko-Man Islands of Siam. In an interesting paper read before the Society of Arts, on the 27th of January, 1871, Lieutenant-Colonel Romaine Wragge produced unquestionable evidence of the existence of peat in many parts of India; and in China † it is certain that peat exists in one locality at least, and it is not improbable that, if sought for, it would be discovered in other parts, also, of that country.

Enough has now been stated on this portion of our subject to prove that peat exists almost universally, wherever circumstances favourable to its existence are to be found. The next point for consideration is the chemical constituents of peat, for upon this depends the possibility of its being converted into a serviceable fuel. Subjoined are some of the most important analyses of peat obtained from various quarters.

The specific gravity of peat varies considerably, according to the nature of the peat moss. Its density varies also with the degree of its dryness. Ordinary peat, according to M. Bosc, is about 170 kilogrammes per cubic metre, and compressed peat from 600 to 800, and sometimes 900 kilogrammes per cubic metre.

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* Page 148.

† "Industries de l'Empire Chinois," par M. Paul Champion, page 10.

Dr. Percy, in his work on Metallurgy, gives the following particulars of peat from Ireland (four samples), and of peat discovered by Dr. Hugh Falconer on the banks of a lake in Cashmere:—

Locality.	Density.	Carbon.	Hydrogen.	Oxygen.	Nitrogen.	Ash.
Ireland:—						
Philipstown .	0·405	58·69	6·97	32·88	1·45	1·99
Ditto . . .	0·669	60·48	6·10	32·55	0·88	3·30
Wood of Allen	0·339	59·92	6·61	32·21	1·25	2·74
Ditto . . .	0·639	61·02	5·77	32·40	0·81	7·90
Cashmere . .	Water. 10·40	32·28	3·66	21·03	1·81	29·80

The following are analyses by M. Regnault:—

Locality.	Carbon.	Hydrogen.	Oxygen.	Ash.
Vulcaire, near Abbeville .	57·05	5·63	21·76	15·58
Longprès	58·09	5·95	2·13	14·61
Champſen (Vosges) . . .	57·79	6·11	20·97	15·35

We have here purposely limited ourselves to a notice of the analyses of peat of the best quality—with the exception of that from Cashmere. The latter will afford some idea of the wide range in variation of constituents which may be found in peat. We now give one or two analyses of the component parts of peat obtained by distillation:—

Locality.	Charcoal.	Tar.	Ammoniacal Liquid. and Water.	Gas.	Ash.	Loss.
Vesle (Marne).	15·45	6·80	38·90	18·60	19·25	1·00
Kœnigsbrunn .	24·40		70·60		5·00	—
				Nitrogen.	Carbonic acid.	Carb. of Hydrog.
Baviere . . .	40·25	24·50	14·10	0·27	10·80	10·18
				Gas and loss.	Cinders.	
Brunswick . .	33·60	5·00	35·00	20·00	6·40	—

The lower the stratum of the bog from which peat is extracted, the more valuable it is as a fuel. The upper surface of a bog is often styled *turf* to distinguish it from the *peat* which lies below. When peat is cut in the ordinary manner, with the view of being burnt as fuel after having merely been dried, and without any further preparation, a particular shaped spade * is used, which cuts the peat at once in blocks of the required size, and these are then stacked in small open heaps to dry in the air. Where there is much water in the cuttings, dredgers of various sorts are used for raising the peat. The object of the present paper, however, not being to describe antiquated processes, but rather to take some notice of the modern improvements in the treatment of peat which the recent high price of coal has called forth, we shall abstain from any further remarks upon the preparation of what is known as "ordinary" peat, that is, peat simply cut and dried in the open air, without any further attempts at artificial preparation.

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* At the Government works, at Haspelmoor, in Bavaria, much of the turf used as fuel in the locomotives of the state railways is less, on an average, than two inches in thickness; the object of this being to facilitate and expedite the process of drying. At an Agricultural Exhibition, at Munich, the attention of the Irish Commission was attracted to a double plane, intended for cutting two sods of turf at a stroke, which they considered well suited for raising thin sods.

To obviate the natural inconveniences arising from the use of raw, or "ordinary," peat, attempts were made in 1821 to compress it into blocks. This plan was first commenced in Germany; afterwards it was adopted in Sweden, at the iron mines of Eckman; subsequently it was introduced into France; and, lastly, it was brought to England about the year 1837. Numerous processes have been patented for the purpose of compressing peat, but they have been unattended with any satisfactory results. At first attempts were made to compress the peat when in a partially dry state, but this proved a failure, as it did not achieve the desired end. Attention was next directed to means for compressing it whilst moist, and for this purpose powerful hydraulic presses were used; but they, instead of driving out the moisture, tended to confine it within the fuel. This plan failing attempts were made to substitute chemical agencies for the expulsion of the water, and the formation of the peat into compact blocks; but, as might have been expected, such treatment would no more produce the desired results than force unscientifically applied as above referred to.

At Haspelmoor and at Kolbermoor, in Bavaria, there are works in which compression of peat by force is, however, carried out at the present day. The system, technically known as "Exter's," consists in obtaining the peat from the surface of a bog (previously well drained and levelled) in the condition of a fine mould or powder, which, when partially dried by exposure to the sun and air, is scraped together in thin layers, and removed to the place of manufacture, where, being dried more fully by artificial heat, it is subjected to powerful mechanical compression effected by steam machinery. The cost of this compressed turf for the past year was estimated at about equal to 12s. sterling per ton. This system is essentially the same as that for which works were at one time erected at Derrylea, in Ireland, where the results were commercially unsuccessful. At

Kolbermoor the bog surface is harrowed by portable steam power, and the exhaust steam from the fixed compressing engine is employed in drying the fine turf mould. These factories are occupied chiefly in producing compressed turf for locomotive purposes.

With respect to the efforts that have been made from time to time for depriving raw peat of its water by hydraulic or other mechanical pressures, it may be stated that all such attempts have been entirely unsuccessful. This is not to be wondered at when we consider the nature and properties of peat.

According to different authorities raw peat, when freshly dug, contains from 75 to 90 per cent of water, and 100 tons would therefore only produce from 10 to 25 tons of fuel—more generally the former quantity than the latter. How to get rid of this water in a cheap and expeditious manner was, therefore, the important point requiring solution.

Peat consists of a number of stems or stalks of the *Sphagnum*, or other peat plant, closely matted together with partially decomposed vegetable matter. These stems all contain capillary tubes, which hold the water with great tenacity, and from which it can only be expelled by the complete destruction of the capillary. Hence we see that the only true method of treating peat is by maceration and precipitation, for it is found that when thoroughly macerated the precipitated particles, uniting themselves by a chemical affinity, discovered by M. Chaleton, a French chemist, causes the pulp to discharge the water contained in it, and to obtain a density nearly the same as pit coal.

This method of preparing improved fuel from peat is based upon the fact that when raw peat is subjected to any treatment by which, in the wet condition, its fibrous structure is broken up so that the whole forms a homogeneous mass of pulp, it will not only dry more rapidly, but will acquire a cohesion and density in the process of drying greater than any ordinary mechanical pressure could impart to it. This quality of density gives not only an increased value to turf as fuel, but also the great advantages arising from increased facilities of transport. Turf, thus treated, on being thoroughly dried, will sometimes become reduced to one-sixth part of its original bulk or volume.

The principle of maceration in the preparation of dense turf is found in the making of "hand-turf," as practised in different parts of Ireland. The capability of hand labour for the production of large quantities of dense turf is, however, limited in practice. Dense turf is produced in large quantities in the Netherlands in the following manner, but the product is not equal to that resulting where machinery is employed for the purpose:—Owing to the position of several peat districts in the low countries, much of the peat lies under water, and, when raised by dredging, is conveyed to working places, where it is well kneaded or trodden under foot, and at the same time freed by hand of any roots or other substances that would interfere with bringing the whole to a fairly uniform mass. The spreading ground is generally strewn with some loose, dry material, such as broken reeds, and in this way adhesion to the surface is prevented, and some opportunity is given for the escape of moisture from below. In the kneading operation short pieces of boards are attached under the feet of the workmen, and, when in this manner the mass is sufficiently levelled, it is marked out lengthwise and crosswise, and is subsequently divided by a simple tool made for the purpose, after which the usual process of drying in the open air proceeds. In Friesland and the district of the Haarlemmer the size of dense turf made in this manner averages, when dry, from 5 to 6 inches in length, and in thickness from 2½ to 3 inches square. In other parts it is produced of reduced thickness with a view to drying in a shorter time, and made thus it has a shape not unlike flat tiles of peat, varying in size from 5 to 6 inches in length by 4 inches in width, and only 1½ inches in thickness. The annual production of dense turf in the Netherlands supplies largely the place of coal for many industrial purposes, and presents the most extensive development of dense turf industry at present in Europe.

For the purpose of producing dense turf of a still better class than the foregoing the application of machinery is required for the complete reduction of the peat fibre. This process was, according to M. Bosc, first employed about fifteen years ago by the Comte de Lard, in the peat-bog of la Saussaye, in the department of Seine-et-Oise, near Paris. The method adopted by him may be thus briefly described:—The peat is extracted from the bog in the ordinary manner. It is then cast into a grinding machine, and ground up with an alkaline solution. The sludge passes into a large basin, and by the aid of powerful pumps it is raised into a second basin, 3 metres higher than the former one; from this it is permitted to flow into open wooden side frames standing on a bed formed of bundles of osier, straw, or grass, through which the water filters away, whilst the pulp left behind attains, at the end of a few days, such a consistency as to allow of its being cut into blocks, and after a further period of fifteen days, the latter are sufficiently dry for stacking. A system of maceration and precipitation, accompanied by filtering, is adopted in the factory of Montanger, near Corbeil; and at a factory near Munich the maceration is followed by compression.

About six or seven years ago works and machinery were erected in the province of Drenthe, in the Netherlands, for the production of dense turf. Here the macerating mill is a vertical cylinder, about 6 feet high and 3 feet in diameter, in which a vertical iron shaft revolves. Upon this shaft several arms are fixed, which tear up the raw peat, after which the pulp is forced by a screw through a pipe at one side. This pipe terminates in a mouthpiece with three orifices, through which the disintegrated peat finally issues forth, and as it issues is cut off when about 12 or 13 inches long, and the pieces are removed for drying in the open air. The dense turf produced by this arrangement is of very good quality; the pieces when dry have an average size of 9 inches long by 2½ inches square, and each piece weighs nearly a pound and a half.

The manufacture of peat has for some years past been extensively carried on in Canada, by a process invented by Mr. James Hodges. The whole of the machinery employed in this manufacture is carried on vessels, which float in canals cut by themselves through the peat bogs. A pair of large screws, with cutting blades, are placed at the front end of the boat, and driven through gearing by an engine in the stern of the vessel. These screws cut their way through the bog, forming a channel 19 feet wide, and from 4 feet to 6 feet deep, and as the water flows in as fast as the peat is taken out, the vessel floats and moves onwards as the screws advance, generally at the rate of about 15 feet per hour. The peat is cut and driven by the screws into a well in the bow of the boat, it being first cleared of any pieces of wood, roots, or other extraneous matter. From the well the peat is lifted by an elevator, and discharged into a hopper, and thence into a part of the machinery which arrests all roots, &c., which have not been previously removed. After this it is pulped, and from the pulping machinery it passes, with a consistency of thick mortar, through a distributing shoot projecting at right angles to the vessel, whence it falls on to a space of ground, specially prepared, on either side of the canal, over which it is spread evenly to a thickness of about 9 inches. In a couple of days or so the pulp is sufficiently dry for the next operation, namely, that of cutting the peat transversely, which is done by hand with tools specially adapted for the purpose. A few days afterwards it is in a fit condition to be cut longitudinally; the size of the slabs, or bricks, being 18 inches long by 6 inches wide. A fortnight later—if the weather be favourable—the bricks are hard enough for stacking, and after several days more they are turned and re-stacked; ultimately they are loaded upon barges on the canal, and floated down to store. The machine, which we have thus briefly described, in ten hours' working excavates and pulps sufficient peat to give 50 tons of air-dried fuel, and in doing so it makes a navigable canal 150 feet long, 19 feet wide, and 5 feet 6 inches deep. One ton of this peat fuel measures 70 cubic feet. Experiments as to its efficiency have been made upon the Grand Trunk Railway of Canada with the following results as compared with coal and wood, with an express passenger train on a run of 177 miles:—

Average mileage run with	1 ton of coal	59·91
,, ,, ,,	1 cord of wood (4000 lbs.)	40·69
,, ,, ,,	1 ton of peat fuel . .	50·50

Taking the then relative prices of the above three classes of fuel it was found that the cost for the distance of 177 miles would be as follows:—viz., coal 29·50, wood 30·87, and peat fuel 16 dols.

It will, of course, be understood that this method of manufacturing fuel can only be carried out where a sufficient supply of water exists in the peat to fill up the channel as it is formed, and to float the manufacturing vessel forward. In the absence of a sufficiency of water for the above purpose, recourse must be had to the use of stationary and fixed machinery. Of this class there exist two principal methods of treatment: the one patented by Messrs. Clayton, Son, and Howlett, and the other the invention of Mr. John Box. These two, however, differ very materially from one another, as will be seen from the following descriptions of them:—

According to Messrs. Clayton's process, the raw peat, as dug, is filled into a special arrangement of "squeezing" trucks, having perforated sides for the escape of water from the peat. A piston forced against the peat in the truck by the aid of a screw and lever effects a pressure upon the body of the peat, and during the passage from the bog to the machine the peat is thus freed of a considerable portion of the water. The rough peat is fed from the squeezing trucks into a hopper, through which it falls down a vertical chamber in which revolves a shaft, having screw blades fixed on it, as in an ordinary pug mill. By the action of these blades the peat is broken up and forced downwards into the comminuting apparatus. The latter consists of a horizontal cylinder fitted with a central revolving shaft, upon which are fixed propelling screws, and a series of curved arms or discs, so arranged upon it, that in their whole length they form a dissected double helix, with increased spiral. Along the bottom of this cylinder, and projecting upwards towards the shaft, are arranged cutting blades of hardened steel, between which the discs pass in their revolution. The general arrangement of this part of the apparatus is very similar to that of a huge sausage-making machine.

The peat thus fed into the cylinder is carried forward by the discs, each revolution bringing the peat against the cutters, and thereby effecting a complete mastication of its fibrous tissues and cellular structure, the roots and other undecomposed portions being reduced to a state of fine pulp, and the whole mass is brought into a uniform condition. The end from which the pulped peat issues is fitted with a number of moulding orifices through which it is forced. These may be of any desired shape and number according to circumstances. Beneath the chamber upon which the moulding orifice is fixed is a roller table, on which the trays for receiving the moulded peat are placed in succession by a bog, so that they run in a continuous series underneath the moulding orifices and receive the peat issuing from them. As the front end of each tray comes up the workman severs the streams of moulded peat by means of a sliding cutter, which severs each bar into pieces 5 inches long. The trays, thus loaded, are lifted on to racks, where they remain for about three days, until the peat will bear handling, when they are placed upon open shelves for final drying. It is stated that peat of a fibrous nature when treated by this machinery becomes compact and hard, and assumes a specific gravity of from 1·05 to 1·10, whilst black decomposed bog condenses to about 1·20.

In conclusion we have to make a few observations upon Mr. Box's method of treating peat in order to produce it in a dense form; but first we must refer to a few principles laid down by that gentlemen to be observed on this subject, as, so far as we are aware, they have not previously been insisted upon. With regard to the initiatory pulping of the peat, it must be observed that this process may be carried to too great an extent, as extreme fineness of maceration entirely defeats the object in view. Mr. Box has remarked on this subject, in a letter to the *Freeman's Journal* of the 24th of January last, that he had seen mantel-shelves made of peat finely passed through sieves, and that it was as little subject to fire as an ordinary piece of stone. It must be remarked also that dense peat ripens by age, and acquires a greater value in proportion to the length of time it has been made; thus, in May, 1872, advertisements in the journals of the Department of the Somme, in France, quoted the price of peat fuel of 1871 at 16s. 4d. per ton, whilst that of 1870 was 18s. per ton. Peat, if properly prepared, may be used after about two to three months' drying, but it is not then hard enough to bear the weight of iron ore in a blast-furnace, nor would it resist the blast for a sufficiently prolonged period of time.

The chief peculiarity in Box's process is, that he employs a Carr's disintegrator in preference to any other kind of mill for pulping the peat; and this he has adapted to the tearing up and disintegration of raw peat mixed with water, it being found that it would not disintegrate the peat unless it was worked in water, and this is the principal change which he has made in Mr. Carr's mill.

With this machine the raw peat is torn up and divided as it comes from the peat bog, the supply of water to it being so regulated as to produce a pulp of a certain and equal consistency. The raw peat is supplied to the side of the opening of the mill by a hopper or funnel of a peculiar form; it is allowed to escape from the mill with considerable velocity, and falls a height of 4 or 5 feet on to a sieve of considerable dimensions, and rushing through it leaves all undecayed vegetable remains on its surface, from whence they are roughly raked by a man placed at its side. The pulp runs into reservoirs, which are merely spaces of ground, surrounded by planks, about 60 feet long by 40 feet wide; the bottom of these reservoirs is specially prepared, and rendered porous by under draining. The peat pulp is run into these reservoirs to a depth of about 9 inches, and within twelve hours it will often be found to have shrunk to 5 inches in thickness, and to have assumed about a consistency of prepared brick clay; at this moment a stamper, worked by a man, cuts the solidified pulp into pieces 9 inches by 4 inches. These pieces shrink from one another, and about the third day they are sufficiently dry to be handled. They are then carried away in specially constructed wheelbarrows, and placed on shelves formed of laths in small sheds or frames, some 400 to 500 feet in length, 6 feet high, and 4 feet deep, where they are left to harden. This hardening, in summer time, takes about two months, and the peat blocks are then ready for use.

From the particulars above given, it will readily be assented that, in the application of the principle of macerating the raw peat, and in the air-drying process, will be found the only reasonable system of producing dense turf upon a commercial basis: and this is the conclusion to which Mr. Purdon's Committee in Dublin arrived. The simple air-drying of raw peat, as it is cut in blocks, with the slane from the peat bed, produces but a light friable species of fuel, and one which is inconvenient for transport, and subject to considerable waste in handling. The more fibrous the peat is, the more subject is it to both these inconveniences. The maceration of the peat into a pulp is, in effect, the hastening of the work of Nature; and its subsequent precipitation causes the production of a result in a few days equal in effect to what would have taken probably centuries in the ordinary course of events.

The question of cost of producing dense peat by maceration and precipitation has purposely not been treated of in the present article, as it has not yet been carried out to a sufficient extent in this country to enable reliable data to be obtained; besides which, many considerations have to be taken into account which differ largely in various districts, such as the price of labour, local peculiarities, the distance of the bog from the manufactory, and other points which will readily suggest themselves, so that they need not be further dwelt on here. As some guide to the cost, however, it may be as well to state, that at Brandenberg the total cost of labour in the production of dense turf was estimated by Mr. Purdon's Committee at 6s. 6d. per ton; at Prince Schwartzenberg's works near Gratzen, in Bohemia, the cost of manufacture is set down at 6s. 9d. per ton; Hodge's Canadian Peat Company produce peat at 6s. 6d.; the Boston Peat Company at 8s.; at Haspelmoor, in Bavaria, it costs 12s.; Messrs. Clayton, Son, and Howlett estimate the cost by their process at from 3s. 6d. to 5s.; whilst Mr. Box believes, that, by his process, the expense of manufacturing peat will be only 2s. per ton.

V. CHANGES IN THE MOON'S SURFACE,

WITH SPECIAL REFERENCE TO

SUPPOSED CHANGES IN LINNÉ AND PLATO.

By Richard A. Proctor, B.A. Cambridge, Hon. Sec. R.A.S.
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THE study of our moon by astronomers has had for its main purpose the recognition of change, the effects of processes taking place on the moon resembling in some degree those with which we are familiar on earth, and especially of effects due to the existence of life, animal or vegetable, upon the surface of our satellite. It is impossible to avoid the recognition of this fact in the work of even the most systematic and rigidly scientific lunarian astronomers. The charting of the moon would certainly not have been prosecuted with the care and energy actually shown by such workers as Cassini, Schröter, Beer and Mädler, Lohrman, and Schmidt, but for the fact that such researches afford promise of an answer to the question, whether the moon is on the one hand a dead and arid waste, without any signs of motion or of change, or on the other hand a scene where systematic processes of change are taking place, which may or may not be interpretable by us, but the mere occurrence of which would suggest that the moon is the abode of some forms of life.

I propose now to enter into the discussion of the question here indicated, regarding it in the light afforded by recent researches chiefly, but not omitting the consideration of antecedent probabilities on the one hand, or of theoretical inferences on the other. For I hold that, in scientific investigation, a mere array of facts is of little force; it is from facts viewed in their relation to known physical conditions that we can alone hope for useful additions to our knowledge. And we must in a special manner avoid the common mistake of too rigidly directing our attention to the particular subject we are upon, instead of combining a careful scrutiny of that subject with the search for information and analogies derivable from other subjects, and even from subjects which at a first view may appear little connected with the one in hand. This, at least, is the course which experience suggests as the most effective, the whole history of science showing the uselessness of the mere accumulation of facts, and the importance of the comparison of the discovered facts of one subject with the recognised truths of other and especially of more advanced subjects.

One other point is to be noticed before we proceed. The enquirer into such a subject as that we are upon, whether he endeavours to advance by means of his own personal observations directly, or by the analysis and comparison of observations made by himself and others, must not act upon any preconceived ideas as to the result of his work. In particular, he must not consider that success depends on the recognition of signs of lunar activity rather than on the acquiring of evidence pointing the reverse way. He must in fact proceed quite independently, or his labours will be worse than useless; they will be self-deceptive, or at least they will have a tendency to be so. It has been unfortunate that some of our most earnest lunarians have adopted apparently a different view, and would seem to consider that, unless they establish the occurrence of change, they have done nothing. On the contrary, if, by the rigid scrutiny of some particular part of the moon, any observer should succeed in demonstrating that there at least there has been no change, or none that can be recognised, his work is as important in its scientific aspect as though he had demonstrated that some remarkable change had taken place, though no doubt far less interesting to the general public.

The first astronomer to take up the special form of lunar research we are here considering, with direct reference to the condition of the moon as a probable abode of life, was Schröter. Fontenelle had earlier discussed the question, and had spoken of the downfall of a lunar peak, which had probably never had any existence but in his own lively imagination. Huyghens, in his "Cosmotheros," takes the habitability of the moon for granted, and speculates fancifully on the nature of the lunar inhabitants; but Schröter endeavoured observationally to establish the fact. He conceived that he had demonstrated the existence of a lunar atmosphere of appreciable extent. He supposed that he had discovered a great lunar city, north of Maurius; besides lunar canals and roads in other regions. He described the formation of a new mountain on the Mare Crisium, and its eventual disappearance; and he asserted that another had come into existence in Helicon*, and still remained, where previously there had been no mountain.

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* The mountain in question disappears when the moon is full, at least for ordinary telescopes; and it was doubtless in this way as Webb suggests, that Hevelius and Riccioli overlooked it.

Gruithuisen went further even than Schröter had done; but his chief reliance was placed, not upon changes in the moon, but on the existence of certain regular formations. "He collected with the utmost diligence," says Crampton, in his interesting little treatise on the Moon, "all the objects which he beheld on the lunar surface having the appearance of regularity, such as would be shown in the works of man here;" and, strange to say, his search, with the aid of a somewhat wild imagination to help it, was by no means unsuccessful, at least as far as "finding such objects" was concerned. Yet objects of the kind appear regular only when examined with low telescopic power. In instruments of considerable size they show manifest signs of being simply natural formations.

However, it is not my present purpose to consider those features of the moon which present an aspect suggestive of artificial construction, but to deal specially with the instances or supposed instances of change.

No one can study the moon for any considerable time without being led to the conclusion that her general aspect is unvarying, save, of course, as respects the changing amount of illumination as the lunar month proceeds. It becomes clearer and clearer, the longer the study of the moon is continued, that it is by the careful scrutiny of small portions of the moon's surface that any signs of change are to be recognised.

But this general constancy is in itself a most important point in our subject. For supposing we should recognise signs of change in any small portion of the lunar surface, we shall have to enquire into the probable cause of such change; and in making such an enquiry, it will be most important to have determined beforehand whether any atmosphere surrounds the moon's globe, and if so, what is its probable nature and extent. I do not propose to discuss this point at any length here, because it is one which has been already discussed in full elsewhere; but I would invite the student of selenography to notice that all the direct evidence tends to show that there is scarcely any appreciable atmosphere round the moon, *not* in quantity (the absolute quantity may be and probably is very considerable), but as respects actual density at the mean level of the moon's surface. Now, in the enquiry into changes taking place on the moon as the result of mechanical processes, whether from the contraction and expansion of the surface, or from sublunarian forces which may still be at work, we are not compelled to consider the density of any possible lunar atmosphere. But if we extend our theories of lunar activity, so as to include changes resembling those due to terrestrial vegetation, or again to those which living creatures might produce by their works, we are bound to take some thought of this relation. We must, in fact, consider how far it is probable that any form of vegetation, or any kind of life can exist, where the atmospheric density is as small, let us say, as in the so-called vacuum produced by one of our most perfect air-pumps. Now it appears unsafe to argue that no kind of life, animal or vegetable, can exist in such an atmosphere merely because our experience has not made us acquainted with any. But, on the other hand, it must be remembered as we proceed, that whatever degree of difficulty there may be in admitting the existence of vegetable or animal life under such conditions, is *opposed* to the occurrence of lunar changes explicable as due to such forms of life. The whole question being one of probabilities, we must not overlook this antecedent improbability.

At the same time, life exists under such varied conditions on our own earth, that it is impossible to assert that, where there is certainly very little air, and as certainly very little, if any, moisture, life cannot exist. Let us admit the possibility, and let us further admit that the strange vicissitudes to which living creatures on the moon would be exposed during the lunar day and night, are not necessarily fatal to the hypothesis that life exists on the moon.

We have, then, two forms of change to enquire into—those due to mechanical, chemical, and other like processes, and those due to the existence of life upon the moon.

But at the outset of the enquiry, we must take into consideration a circumstance which is very frequently overlooked in dealing with this subject. In all terrestrial comparisons to determine processes of change, the observer or experimenter is careful always to keep the circumstances unchanged under which the object of research is examined. If he desired to ascertain whether some distant and (let us say) inaccessible surface underwent changes, he would, to speak plainly, be careful to look at that surface in the same way throughout his experiments, and also to select occasions when the atmosphere was in some given condition.

Now, first, the conditions under which any lunar object is observed necessarily change with the progress of the lunar day. As the sun gradually rises higher and higher above the horizon of any lunar place, the shadows not only decrease in length, but shift in direction; and as the sun passes down again towards the horizon, the shadows, though they increase again in length, are yet thrown in quite a different direction from that in which they fell in the earlier part of the day. The effect of such changes will depend partly on the nature of the surface; but all parts of the moon's surface where one would look for changes due to volcanic action, or to the effects of expansion and contraction, would be certainly very much affected by changes of illumination. Thus it is found that the whole aspect of a lunar region at morning time differs from its noon aspect, and its noon aspect again from the aspect it presents when its evening is in progress. We can take the diurnal changes into account in successive lunations, because (weather permitting) we can observe any given lunar region repeatedly at about the same hour of the lunar day. But we cannot do this with perfect exactness; for the lunar day, that is the lunation, is not commensurable with our day. Since one lunation in fact contains approximately 29·53 of our mean days, we see that if any lunar feature is observed in a given part of our sky, at a given lunar hour in one lunation, then, in the next lunation, that part of the lunar day will correspond to a time when the moon is nearly 12 hours of diurnal rotation from that part of the sky. For instance, if true full moon occurs at midnight in one lunation, so that a place on the moon's central meridian is observed at its noon and at our midnight, then, in the next lunation, the noon of that place will occur nearly at our mid-day, and the moon was on the meridian about half a day of our time before, or will be on the meridian about half a day of our time after the time of true moon for places on the central meridian of the moon; in half a day of our time a place on the moon undergoes a considerable change of illumination. Since two lunations amount to 59·06 days, that is to 59 days and nearly an hour and a half, we see, that in the next lunation but one, there will be a much smaller difference of illumination if any lunar region is observed at almost the same hour of terrestrial time; for an hour and a half of our time corresponds to only about three minutes of lunar time,* and as we know the sun's position does not change much in three minutes. But then a great change will have taken place in the moon's diurnal course, simply because the moon's position with respect to the equator, at any given phase, varies as the sun's does (sometimes more, sometimes less, according to the position of the nodes of the moon's orbit, but always to a considerable degree), since the inclination of the moon's orbit to the equator is never less than 18°; accordingly a long time elapses before there is a close approach to identity in the lunar and terrestrial conditions under which a lunar region is observed.

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* Since the lunar day contains 29·53 of our days, it follows that the lunar hour, or the 24th part of the day, corresponds to 1·23 terrestrial days, or 29·53 terrestrial hours. Again, one terrestrial day corresponds to 1 ÷ 29·53 of a lunar day, or to rather less than 48m. 46s. of lunar time supposed to be divided as ours (that is, the day into 24 equal parts, to be called lunar hours, the hour into 60 minutes, and the minute into 60 seconds). These two relations are sufficient for the ready conversion of terrestrial into lunar time, and *vice versa*.

And it is to be noted that, when so far as the moon's motion in her orbit is concerned there would otherwise be a close approach to identity in the conditions, the continual change in the inclination of the orbit causes a marked difference in the elevation at which the moon is seen above the horizon.

If we add to these considerations the fact that the moon has seasons, though they are not very marked, and that the sun's elevation at lunar noon thus varies through an arc of about 3°, we see that a very long interval must elapse before there is any very near approach to the conditions, lunar as well as terrestrial, under which any lunar region is observed.

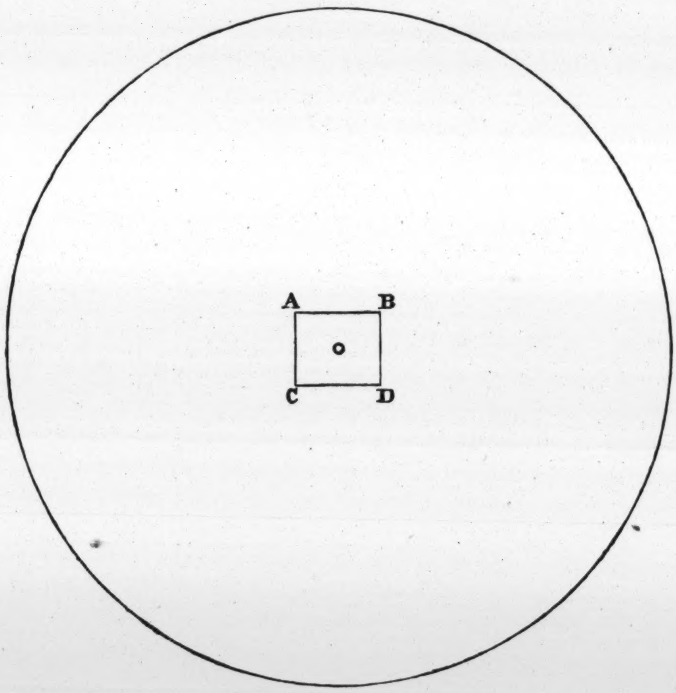
As yet we have taken no account whatever of the lunar librations. These occasion a distinct class of differences. The varying solar elevation affects the actual aspect of any lunar region as it would be seen from one and the same standpoint; and varying lunar elevations, by causing the moon to be observed under different conditions of terrestrial atmosphere, must manifestly produce varying effects. But the lunar librations correspond to an actual change of place on the observer's part.

This would not be the place to give a full account of the lunar librations. In fact, the subject would require much more space than is here available. But there are certain considerations which bear in a very important manner on the question of change in the lunar surface.

Let it be noticed, that the point which is at the centre of the moon's disc when there is no libration is carried by the librations so as to occupy in turn every part of a lunar area appreciably rectangular, some $15\frac{1}{2}°$ wide in lunar longitude and some $13\frac{1}{2}°$ wide in lunar latitude. Thus the lunar region occupied by this point is viewed in every direction corresponding to these limits. We see it square when it is in its mean position, we see it tilted $7\frac{3}{4}°$ on either side of its mean position in longitude, and $6\frac{3}{4}°$ on either side of this position in latitude, and in every possible mean position as well as with every possible combination of tilt in longitude and latitude. In fine, if O (Fig. 1) be its mean and central position, then this point occupies in turn (and in the course of time) every part of the area A B C D.

Now this has only been stated to show the actual librational sway of the moon, not to indicate the importance of the effects due to such libration. For it is manifest that the region about O cannot be very much affected in appearance by being shifted even to the point A, or to B, or to C, or to D, that is, to its maximum amount. If we were looking at the summit and slopes of any hills or craters, when the central region was at O, we should also be looking at those summits and slopes when the region was at A, B, C, or D,—unless, indeed, the slopes were exceedingly steep.

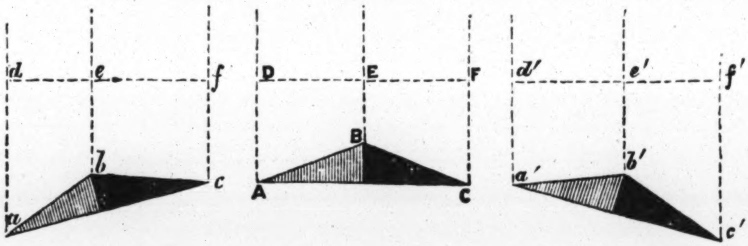
FIG. 1.



Of course the two opposite slopes of a ridge, suppose, would be seen in different proportion at A and C, or at D and B, and if they were differently tinted, a different effect would be produced, whether we could see such slopes separately or not.

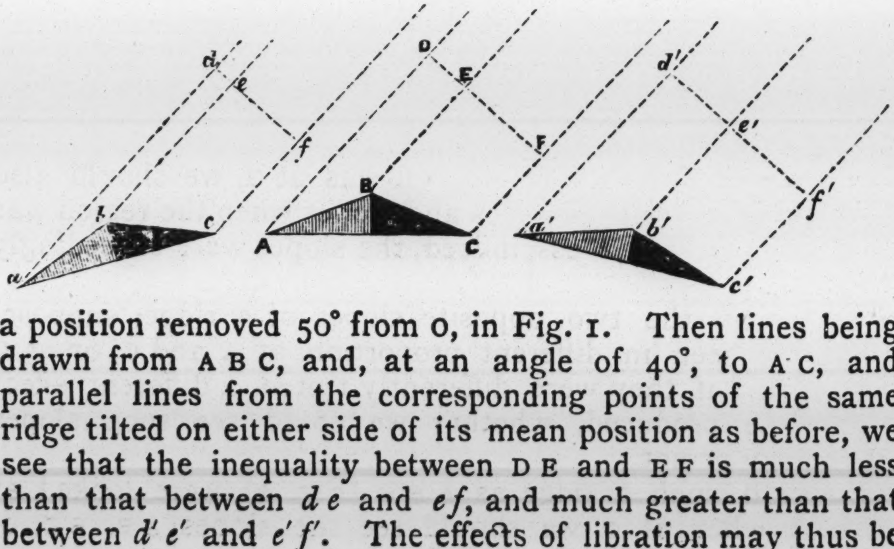
Thus if such a ridge as A B C (Fig. 2) were looked at directly when at O, we should see the slopes A B, B C, of apparently equal width, as shown by the equality of D E and E F drawn square across the parallels from A, B, and C, towards the eye; whereas, if the base A C were inclined to the position *a c* or *a′ c′*, we should in one case see *b c* the wider, and in the other see *b′ c′* the wider, as shown by the inequality of the lines *d e*, *e f*, and *d′ e′*, *e′ f′*. Such changes would necessarily produce some effect; and the effect might be deceptive, and indicate change where there had been no change, if a surface were covered with ridges such as A B C, too minute to be individually discernible, and having the side towards C darker or lighter than the side towards A. The same would hold also if the slopes A B and B C were not, as in the imagined case, inclined equally to the base A C.

FIG. 2.



But it will be manifest that these effects, though they might be appreciable, would be insignificant compared with those which libration might produce, in certain circumstances, on points at a considerable distance from the centre of the disc. Thus, take such a case as is illustrated in Fig. 3, where a ridge, A B C, instead of being looked at squarely, is viewed at an angle of 40°, corresponding to a position removed 50° from O, in Fig. 1. Then lines being drawn from A B C, and, at an angle of 40°, to A C, and parallel lines from the corresponding points of the same ridge tilted on either side of its mean position as before, we see that the inequality between D E and E F is much less than that between *d e* and *e f*, and much greater than that between *d′ e′* and *e′ f′*. The effects of libration may thus be

FIG. 3.



very considerable indeed on places considerably removed from O (Fig. 1). It is indeed clear that places which, when the moon is in mean libration, are near the lunar limb, but not near enough to be carried actually out of view, must be very importantly affected, since *d e* (Fig. 3) would vanish altogether with a slight reduction of the angle at which the parallels of the figure are inclined to A C.

Remembering that every point of the visible lunar hemisphere undergoes libration, and that in every lunar month there is a complete oscillation of the point O over a certain libration-ellipse, which is continually varying in different months as well in position and size as in the direction in which it is traversed, while the maximum libratory effects (always considerable) are attained at different epochs in different lunations, we see that we have here a very important cause of changes in appearance. It is utterly impossible that any surface like the moon's could, as a whole, undergo such remarkable librations without some noteworthy changes of appearance being produced, even without those changes of illumination which have been referred to above. Further evidence on this point will presently be cited.

Now the cycle of libratory changes for the moon, regarded without reference to her phases, is a long one. It amounts, on the average, to almost exactly six years; and we may say that the same libratory condition is not restored until this period has elapsed. There are momentary coincidences of position, but these positions are arrived at, and passed away from, in different ways, until at the end of the long cycle the same series of changes is re-commenced. But this is not all. We must consider *phase* in this inquiry; indeed, it is the most important consideration of all. Now, the six-yearly period brings back the same libratory condition, but not the same phase when given libration effects are produced. This is manifest if we consider that the node regredes only 19° 21′ per annum, and therefore in six years regredes little more than 116°, having, therefore, a totally different position with respect to ecliptical longitude. In two six-yearly periods it regredes rather more than 233°, or has still a totally different position. In three six-yearly periods it regredes 348° 23′ 24″, or is now 11° 36′ 36″ from its first position. This is the nearest approach. The fourth, fifth, and sixth six-yearly periods bring the node to 23° 13′ 12″ from its first position. We may call this a second eighteen-yearly period. Ten such eighteen-yearly periods bring the node 116° 6′ from its first position, and one other six-yearly period then brings it into all but perfect coincidence with its first position. But 186 years have then elapsed, and though the conditions are nearly reproduced so far as libration is concerned, the astronomer who made a first series of observations at the beginning of this period is not alive to repeat them under like conditions, even if like conditions existed. Even this, however, is not the case absolutely, since the lunation would be in another part of its progress at any given season of the year, at the end of the long period named.

Of course I would not have it understood that there is not, within much shorter periods, an approach to the restoration of given relations. Two or three times, perhaps, in ten years, any given feature in the moon may be seen under conditions so nearly alike as to produce a great similarity of aspect; and if the weather is favourable on such occasions, a legitimate comparison may be instituted for the purpose of ascertaining if change has taken place. But it may safely be asserted that the opportunities presented during the life of any single astronomer for a trustworthy investigation of any portion of the moon's surface under like conditions are few and far between, and the whole time so employed must be brief even though the astronomer devote many more years than usual to observational research.

I shall now proceed to indicate a remarkable instance of the effects produced by libration on the aspect of a lunar region lying not very far from the limb, in order that the student of the subject may duly recognise the importance of *position* in this matter.

Mr. Webb, in his "Catalogue of Lunar Objects," makes the following remarks respecting a supposed lunar crater:—"On the western edge of the Mare Crisium Schröter delineated a crater, called by him Alhazen, which he employed to measure the existing libration; he saw in it, after a time, unaccountable changes; and now, it is said, it cannot be found. Beer and Mädler confounded it with a crater lying further south; the question, however, which in the interim was debated between Kunowsky and Köhler, is not quite cleared up." In January, 1862, Mr. Birt observed two objects where Schröter had seen a single crater, and addressed a letter on the subject to Mr. Webb, who then gave him the following interesting and instructive history of the region:—"Schröter had watched Alhazen and measured from it for years, and found it too varying in aspect to be accounted for by the varying angle of illumination. At first it was a depressed circle surrounded by a ring, and distinguished from the neighbouring objects under all angles by its dark grey tint. Subsequently, it often appeared, even in a 27-foot reflector (mirror about 20 inches) under favourable circumstances as a bright longish flat mountain, though *more* frequently in its original grey aspect; occasionally it would be so indistinct, other objects being well defined, that he could not tell what to make of it. On March 1, 1797, Alhazen being very near the limb (only 27·27′ from it), and therefore in a position to be very indistinct, especially as the terminator had advanced to the other side of the *Mare*, he saw it with a 13-feet reflector (mirror about 9 inches and power 180) more distinctly than ever, and in quite a new form, as a real very deep and bright crater, with an irregular ring, scarcely united to the south, and open to the north, with a projection on the east side. Also, there was a small shadow as of a crater never seen before during the innumerable observations of ten years. Schröter thought Alhazen, under this aspect, appeared as deep as Proclus." "Mr. Webb," proceeds Mr. Birt, "enclosed a tracing, with this remark:—'I think you will consider it as affording an interesting comparison with your own observations. He has figured, you will see (as well as described), a little crater where you describe two (?). The circumstance of his ranges uniting so closely to the south may be due to a different libration.' Kunowsky, in the *Astronomische Jahrbuch* for 1825, speaks of Alhazen as lost. In the *Jahrbuch* for 1826, Pastorff, writing January 20, 1823, says his son repeatedly found Alhazen. Pastorff also saw it. In the *Jahrbuch* for 1827, Harding is recorded as having seen Alhazen as Schröter had drawn it. Pastorff saw it in the same year, and in 1829. The difference of aspect, as well as the occasional difficulty of finding this interesting spot, is highly curious. Schröter's earliest delineation gives it, as described, a shallow depression, *entirely* surrounded by a ring. My own observations, which now follow, may perhaps throw some light on these differences and difficulties. Two ranges of mountains, one behind the other, may easily be taken for a crater, and at a certain angle of illumination it may be exceedingly difficult to distinguish the difference. After a while the supposed crater entirely vanishes, libration alters the visual angle, and rotation the illuminating one, the observer being greatly puzzled as to what has become of his well-recognised crater."

Mr. Birt then describes the observations made by himself on January 3 and 4, 1862. On the former day Schröter's

Alhazen at first sight appeared to have somewhat the appearance of a crater, the west edge being high but the east much lower. "Upon attentively considering it," says Mr. Birt, "I have some reason to think it consists of two nearly parallel ranges of mountains, just on the borders of the *Mare*, the eastern range forming a part of the actual border. The shadow of the western range is, under this illumination, terminated at a line west of the eastern range, the western slope of which is glowing in bright sunshine." On January 4, Mr. Birt remarks as follows,—"At times the definition has been very fine, and the real character of Schröter's Alhazen well seen; the southern terminations of the two mountain ranges were seen to be quite separate the one from the other, and the level surface passing between them. It is not surprising that the two combined should have been regarded as a crater, especially if viewed by a low power; for now, haze coming on, it is impossible to distinguish the two from a crater. In the earlier part of the evening, the independence of the two ranges, especially on the south, was very apparent; the *shorter* shadow brought out very distinctly the mountain character, and the recess of the shadow of the eastern range revealed the existence of two (?) small craters lying at the foot of the eastern slope, upon the very border of the Mare Crisium. Beer and Mädler figure two mountain ranges in the locality, but very unlike the mountain ranges described above."

All this is very instructive. It shows what effects the varying visual angle and illumination will produce where most effective, and therefore the effects which they tend to produce under other circumstances.

I pass to the consideration of two lunar regions, to which in recent times attention has been specially directed,—the crater Linné and the Floor of Plato,—proposing to discuss the evidence of change (mechanical change in the one case, and systematic variation recurring each lunar day in the other), with careful reference to all the known circumstances of each case.

On November 17, 1866, Mr. Birt received a letter from Dr. Schmidt, of Athens, to the following effect:—"Depuis quelque tems je trouve qu'un cratère de la lune situé dans le plaine du Mare Serenitatis, n'est plus visible. Cette cratère nommé par Mädler 'Linné' se trouve dans la quatrième section de Lohrmann sous le signe A. Je connais ce cratère depuis 1841, et même en pleine lune il n'était pas difficile de l'apercevoir; en Octobre et Novembre, 1866, à l'époque du maximum de son apparence, c'est à dire un jour avant le lever du soleil à son horizon, cette profonde cratère, dans le diamètre est 5·6 milles Anglais, était parfaitement disparu ; seulement un lueur, un petite image blanchâtre se présentait au lieu de Linné. Auriez vous bien la bonté de faire quelques observations sur cette localité."

Passing over the earlier observations made in accordance with this suggestion, I quote the statements made by Dr. Huggins, in the "Monthly Notices of the Astronomical Society" for June, 1867. He remarks that "on May 11, 1867, Linné had the appearance of an oval patch on the darker background of the *Mare Serenitatis*. The character of the surface of the white spot may be described as similar in appearance to that of a cloud, for it presented no distinct details, and remained undefined when the small neighbouring craters were seen with great clearness. The absence of any defined points upon which the eye can rest is probably the reason that the boiling motion of our atmosphere is perceived in a much more marked manner over the white spot than on the adjoining sharply-defined parts of the moon's surface. From this cause, Linné appeared on several occasions as a mass of white cloud in motion, at the same time that the craters near it were seen steadily and with distinctness. This cloudy appearance arises probably from a peculiar partly reflective property of the material of which Linné consists. Some other portions of the moon's surface reflect light in an analogous manner. . . . The shallow saucer-like form of Linné was not seen, but I have detected it on other occasions. . . . In the centre nearly of Linné, but rather nearer to the western margin, was seen the small crater. This object was well defined in the telescope. The interior of the small crater was in shadow, with the exception of a small part of it towards the east. The margin of the small crater was much brighter on the western side, and at this part appears to be more elevated above the surface of Linné. Under very oblique illumination, this high western wall appears as a small brilliant eminence, and casts a shadow which is somewhat pointed. . . . I estimated the diameter of the small crater to be rather greater than one-fourth of the diameter of the white spot." . . . . On the evening of July 9, the boundary of Linné was noticed not to end abruptly, but to pass "gradually into the darker surface of the Mare Serenitatis."

Dr. Huggins then proceeds to inquire into some of the historical evidence. "Herr Schmidt," he says, "is of opinion that a great change has recently taken place in the appearance of Linné, when it is viewed under oblique illumination. This conclusion is based upon a comparison of its present appearance with the descriptions of Lohrmann and Mädler, and with Herr Schmidt's own observations from 1841 to 1843. On this account, it is of importance to notice that the earlier observations by Schröter seem to agree very closely with the appearance which Linné now presents. In Plate IX. of Schröter's "Selenotopographische Fragmente," the place occupied by Linné is marked by a round white spot, and not by the figure of a crater. The spot is distinguished in the plate by the letter *v*. At page 181, Schröter gives the following description of this object:—'Die sechste Bergader Kommt von einer fast dicht an den südlichen Gränzgebirgen befindlichen, verhältlich gezeichneten Einsenkung *u*, streicht nördlich nach *v*, woselbst sie wieder eine ohnegefähr gleich grosse, aber ganz flache, als ein weisses, sehr kleines rundes Flecken erscheinende, etwas ungewisse Einsenkung in sich hat.' . . . The observation was made in 1788, November 5, from 4 hours 30 minutes to 8 hours. The mean time of the observations was 7 days 14 hours after new moon. Schröter employed a power of 161 on his 7 feet reflector. The description of this object as a flat somewhat doubtful crater, which appeared as a round white spot, agrees remarkably with the appearance which Linné now presents under similar conditions of illumination. The absence of any mention by Schröter of the small interior crater cannot be regarded as evidence of much weight that this little crater has been subsequently formed. An object so small might easily have been overlooked by Schröter. However, Lohrmann's description, in 1823,* and that by Mädler, in 1831, do not appear to be in accordance with Schröter's observations, or with the present aspect of the object. My observations were made with a refractor of 8 inches aperture, and with various powers from 200 diameter to 800 diameters."

The next communication from which I shall quote is contained in a paper in the *Student*, August, 1867, and is by Mr. Birt:—

"The question of change on the moon's surface, supposed to have been manifested in the case of the crater Linné, with which our readers are acquainted, remains *undecided*. Respighi, on the Continent, as well as several eminent astronomers in our own country, having come to the conclusion that no change whatever has taken place in the condition of Linné, and that if any appearances have been presented indicating change, such appearances are to be explained either by defective observations, by unfavourable conditions of our own atmosphere, by variations in the angles under which we see lunar objects, or by different incidences of the solar light falling upon them.

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* "A is the second crater upon this plain,—has a diameter which exceeds somewhat one mile, is very deep, and can be seen under every illumination." —*Topographie der Mondoberflache*, p. 92 and plate, section IV.

"The results that have as yet been arrived at, and which are supported both by English and Continental observations, are as follows:—

"First. The existence of a *shallow* crater, usually presenting the appearance of a whitish cloud, which, by the way, is of variable size; the crater itself has been very rarely seen. Respighi saw it on the 10th of May, 1867, during a perfectly tranquil state of the air. Knott caught a sight of the ring on January 12th, 1867, and, on the same evening, in moments of quiet air and good definition, Buckingham noticed the shallow depression. Webb saw the ring on April 11th, 1867.

"Second. In this shallow crater or depression, a little west of the centre, a small crater with a well-marked interior shadow has been seen more or less distinctly, both in England and on the Continent, since November, 1866; in some cases a perfect crater, in others portions only have been detected. The evidence tending to establish the existence of this small crater is certainly beyond dispute.

"Third. Herr Schmidt, of Athens, carefully observed Linné from October 16th, 1866, and during November, 1866, without having detected either the large *shallow* crater or the small one within it. The rim of the small crater appears to have first arrested his attention on December 13th, 1866, as a delicate white hill; Buckingham seems to have first seen the shadow as a black spot on the following evening, December 14th.

"From the data given, a table (see next page) has been constructed.

"Respighi would seem to have measured the shallow crater instead of the small one."

Wollf makes the following remarks in the *Comptes Rendus* for June 17, 1867:—

"Since the 10th May I have noticed that the crater Linné continues to exist, but with a much smaller diameter than that of the crater indicated in the maps of Lohrmann or Beer and Mädler. In the centre of the white spot a circular black hole may be seen, bordered on the west by a portion of ground which seems prominent above the

ESTIMATIONS AND MEASURES OF THE EXTENT OF LINNÉ.

Authority.	Epoch.	Eng. feet.	Seconds.	Objects.	Remarks.
Schmidt . .	—	36,449	5·17	Crater.	
B. and M. .	1831	33,482	4·83	Crater.	
Schmidt . .	1866, Oct. 18	48,688	6·90	Whitish cloud.	
Birt . . .	1866, Dec. 15	81,920	11·61	Whitish cloud.	Measured.
" . . .	1866, " 18	49,886	7·07	Whitish cloud.	Measured.
" . . .	1866, " 19	51,650	7·32	Whitish cloud.	Measured.
" . . .	1866, " 21	47,627	6·75	Whitish cloud.	Measured.
Schmidt . .	1866, " 27	12,790	1·81	Whitish cloud.	
Birt . . .	1867, Jan. 14	56,100	7·95	Whitish cloud.	Measured.
Buckingham	1867, Mar. 14	42,336	6·00	Whitish cloud.	Measured.
Wolf . . .	1867, June 12	31,752	4·50	Whitish cloud.	
Birt . . .	1867, July 8	37,623	5·33	Whitish cloud.	Measured.
" . . .	1867, " 9	49,420	7·00	Whitish cloud.	Measured.
" . . .	1867, " 10	37,845	5·36	Whitish cloud.	Measured.
Schmidt . .	1866, Dec. 13	1,918·4	0·27	Delicate hill.	
" . . .	1866, " 26	1,695	0·24	Fine black point.	
" . . .	1867, Jan. 25	1,279	0·18	Fine black point.	
" . . .	1867, " 25	1,918·4	0·27	Fine white peak.	
Secchi . .	1867, Feb. 11	2,352	0·33	Small crater.	
Respighi .	1867, Apr., May	28,224	4·00	Small crater.	
Wolf . . .	1867, June 12	7056	1·00	Small crater.	

remainder of the spot. This slight extra elevation has already been described by Schmidt. Atmospheric circumstances did not allow me to obtain an irreproachable image of the moon before the 10th June. On that day, at 8 o'clock, Linné had already been in full light nearly 48 hours, and the central hole could be seen with perfect sharpness. It is a deep crater—deeper than most of the little craters surrounding it, if one may judge from the comparative intensity of the shadows; but its diameter is not equal to that of craters A and B of Beer and Mädler. The white spot which spreads radiatingly (*s'étend en rayonnant*) round it, had, on the 12th June, a diameter of 4·5″, that of Bessel being 7·7″; the crater itself subtending a little less than one second. The perfect purity of the atmosphere, and the optical power of the telescope which I employed, allowed a number of small craters to be seen very distinctly round Linné, or rather a number of small round holes without elevated margins, and which are not shown in Beer and Mädler's map. Six of these little craters form a very remarkable double range to the north and north-east of Linné. They are smaller than the craters in a line situate to the north-west of Linné, and noticed by Schmidt. I employed magnifications of 235, 380, and 620 times.

"The brightness of Linné has not changed since Beer and Mädler's observations, for it is always equal to that of the white spot situated near Littrow, on the western margin of the Sea of Serenity, to which B. and M. assigned the luminosity 6.

"If, then, we compare the actual appearance of Linné with the *text* of Lohrmann and his successors, it is possible, *à la rigueur*, to believe that it has undergone a certain change. Linné has always been a deep crater, with elevated margins; its lustre has not changed—its total diameter has remained about the same. A comparison of *maps*, on the contrary, indicates a real alteration, for these figure a large crater occupying all the space now filled by the white spot. Schmidt thinks that we cannot refuse to attribute great weight to the identity of the indications of these two maps. The authors of the second, having the first at their disposal, it is probable that if they had not found the great crater drawn by Lohrmann, they would have noticed so extraordinary a fact. It is not, however, without interest, to compare their indications with that of earlier maps. The picture drawn and presented by Lahire, which is in the library of St. Geneviève, represents Bessel, Sulpicius Gallus, and other little craters, equal to Linné in the map of Mädler; but he does not indicate Linné. He has only many white spots in this part of the sea. Cassini's map appears merely a copy of Lahire with less detail. According to Schmidt's note, Schröter seems not to have seen Linné, at least not as one of the principal craters in the Sea of Serenity, although he noticed others that were smaller.

"If we consult the photographs of the moon, we see, in the large copy of Warren De la Rue (1858), Bessel and Sulpicius Gallus exhibiting an indication of an interior shadow, while Linné figures as a white spot. The same is seen, though clearer, in the enlarged copy of the magnificent photograph obtained by Mr. Rutherford on the 4th March, 1865.

"The disappearance of the great crater of Linné, then, dates as far back as 1858, if not as far back as Lahire. Apart from the indications supplied by the maps of Lohrmann and Beer and Mädler, to which we may oppose the counter indications of Lahire and Schröter, we only possess a single positive document testifying that Linné has undergone any change, and that is the affirmation of Schmidt that his crater and drawings of 1841 represent the object differently to what is now seen."

In April, 1869, Mr. Browning described his observations of Linné, and gave pictures of the shallow crater and the small crater within. These pictures are interesting, as showing the changes which the same object may present, as seen by the same observer, within a very short space of time. The following description is from Mr. Browning's Note-Book. I invite special attention to the times of observation:—

"September 8, 1869. 11.30 a.m.—Saw Linné very distinctly as a crater, elongated north and south. The north part of the crater the narrowest. The evening terminator crossing Bessel. Eye-piece used, positive achromatic, power 208.

"2.15 a.m.—The wall of the crater appears nearly perpendicular on the west.

"3 a.m.—The small crater is seen to be in the centre of a shallow cup, about four or five times its own diameter. Eye-piece same kind, power 306.

"4 a.m.—A white nebulous line, resembling steam, appears to start from the mouth of the crater, and is continued in the form of a scythe, with the blade directed towards the west. At 4.15 this line was much fainter, and at 4.50 it was no longer visible. As this appearance only presented itself for about ten minutes, I have little doubt that it was an optical illusion, caused by a small cloud in our atmosphere, moisture in front of the pupil of the eye, or some cause quite independent of the object." (I venture to doubt whether any such cause could have produced an illusion of the sort lasting for so many minutes). "During the time the appearance lasted," proceeds Mr. Browning, "I changed the eye-piece, and rotated the drawer-tube which carried the eye-pieces. As neither of these proceedings affected the appearance, it could not have been a 'ghost,' caused by reflection from the surface of one of the lenses in the eye-piece."

Mr. Browning, after considering the evidence afforded by his own observations, considered in connection with those made by others, arrives at the conclusion that "there is scarcely any ground for supposing that any change has occurred in this small but celebrated crater. Should this eventually prove to be the case," he adds, "the time that has been given to this matter has not on that account been lost. Attentive examination of this minute object has made us acquainted with peculiarities of the reflecting power of portions of the moon's surface which may ultimately lead to some more exact knowledge of the character of the surface of our most interesting because most perplexing satellite."

These remarks appear to me to contain the gist of the whole matter. We see that Linné has a surface so constituted that as the sun is rising there, and so pouring his rays very obliquely, there is a continual change of aspect precisely resembling that which can be recognised when certain kinds of rock surfaces, and especially crystalline formations, are viewed under oblique illumination. We know that in such cases the tints vary not only absolutely, but relatively, insomuch that a part which is darker than another with one oblique illumination will be lighter under another and but slightly different oblique illumination.

It appears to me that no other explanation can reasonably be suggested; because, in point of fact, we have to choose between the theory that there has been a definite change of surface in this part of the moon, or that the change is only apparent. Now if there has been a definite change at any time, fresh changes must have restored, either from time to time or definitely, the former condition of the surface. But this seems extremely unlikely; while such a change as Sir John Herschel considered to afford the best explanation of Schmidt's observations may be regarded as one which no subsequent process could so modify as to restore, or nearly restore, the original appearance of the region. For, says Herschel, "the most plausible conjecture as to the cause of" the disappearance noted by Schmidt, "seems to be the filling up of the crater from beneath by an effusion of viscous lava, which, overflowing the rim on all sides, may have so flowed down the outer slope as to efface its ruggedness, and convert it into a gradual declivity casting no true shadow." Such a change would doubtless account well for the observed appearances; but it leaves the restoration of the crater afterwards unexplained.

It remains to be noticed that recently the crater has been observed again somewhat attentively by Dr. Erck and Mr. Burton. Mr. Birt thus speaks of their observations of Linné:—"During the present year this celebrated object has appeared more of a crater form than for some years previously, at least since 1866. . . . On June 4, 1873, Dr. Erck measured the largest diameter of the elliptical spot, and found the mean of several measures to be equal to 4 seconds, or 28,226 English feet, which but little exceeds half the length of the largest diameter as measured by Dr. Huggins. Mr. Burton gives a drawing to scale, which differs very materially from Dr. Huggins's, in the "Monthly Notices." In Dr. Huggins's drawing, the small crater is situated in the *western* part of the white spot, its exact position being indicated by the following numbers: Length, 35; small crater, 5, or one-seventh of the length of the white spot; west rim of small crater distant from west edge of white spot, 10; east edge of small crater distant from east edge of white spot, 20. In Dr. Erck's (and Burton's) drawing, the small crater is situated on the *eastern* part of the white spot; its position, measured on the same scale as Dr. Huggins's drawing, is shown as follows: Length, 15; small crater, 5, or one-third of the length of the white spot; west rim of small crater distant from west edge of white spot, 8; east edge of small crater distant from east edge of white spot, 2. Upon reducing the numbers for each drawing to the same scale, we have, in 1867, the eastern distance double the western, and in 1873, the same distance is only one-third that of the western. Equal weights being accorded to the drawings (and we know that, in 1867, the then president of the R.A.S., Professor Pritchard,* laid great stress on the drawings of Dr. Huggins), it is clear that Linné has undergone a change in the interval between 1866 and the present time, and this circumstance of itself is enough to induce renewed energy in as earnest an attack upon Linné as took place in 1866 and 1867, especially as there is now great probability of settling the disputed question of change."

It needs, however, only a comparison between the three drawings taken by Mr. Browning on September 8, 1869, to see that within the space of three hours Linné changed from the aspect presented to Mr. Burton in 1873 to an aspect more nearly resembling that presented to Dr. Huggins in 1867.

I pass now to the Floor of Plato.

It was in November, 1861, that Mr. Birt communicated his first series of observations of Plato to the Royal Astronomical Society. On the same occasion he described an instrument for comparing colours, which he proposed to call the *homochromascope*. In describing this instrument, he remarked that he had "found it necessary to devise some means for comparing with various standards of colour the tints of various portions, especially the dark-floored craters, the extensive grey plains, and the more luminous districts in immediate proximity with the raised craters." I do not quote the description of the instrument, because I am only concerned with its purpose; and, in point of fact, the instrument has not been completed, nor any substitute used in the observations of Plato, so far as I know. What I wish to indicate is that, at the beginning of this inquiry, Mr. Birt recognised the necessity of some instrumental contrivance for determining colour tints, as distinguished from mere processes of eye-estimation.

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* We have evidence here of the mischievous results which must follow from the election to high office in a learned society of a fellow thereof who, whatever his qualifications may possibly be in other departments of knowledge, has no special knowledge of the subject to which such society is devoted. The supposition that any astronomical work by Dr. Huggins could gain weight from comments by Professor Pritchard is amazingly absurd.

The observations of the Floor of Plato divide themselves into two classes. First, there is the observation of the floor itself, regarded as a whole, and compared with neighbouring regions, and especially with the neighbouring parts of the *Mare Imbrium;* then, secondly, there is the study of the spots, thirty-seven in number, which have been detected in the floor, and which vary in visibility, not only absolutely but relatively.

The main result which Mr. Birt deduces from the general study of the Floor of Plato is that it grows darker as the lunar day advances, being darker near noon at Plato. I do not know that any good would be gained by entering into minute details, which would occupy much space, and would not elucidate the subject, since those who believe that a real change takes place attach no importance to these minutiæ, but ask for acceptance only of the general fact that as the sun rises higher above the level of Plato the floor of the crater grows darker as compared with the neighbouring region, and that the morning tints are resumed as the sun gradually passes from its culmination descendingly towards the western horizon of Plato.

I have before me as I write a series of observations made by Mr. Neison, F.R.A.S., which would occupy, were I to quote them in full, far more space than remains available to me. They indicate, as satisfactorily as need be, the process of darkening to which I have just referred.

Nothing could seem more clearly demonstrated than the fact that Plato darkens towards the noon-tide hours of that lunar region. We might enter on the inviting speculations suggested by such a state of things, inquiring whether that darkening was due to some process of vegetation, or to chemical changes in the surface of the floor. And a variety of speculations more or less ingenious might suggest themselves as to the general condition of the regions within the circular crater-walls.

But we must not overlook the possibility that the darkening of Plato may be apparent only, not real. It is most important that before we begin to reason on processes of change we should assure ourselves that change really does take place. There is another reason for caution in the circumstance that, so far as can be seen, any explanation of the darkening of Plato which refers the phenomenon to real processes of change, must be based on the conception of physical processes unlike any with which we are familiar; and it is a recognised rule in science that such conceptions should be avoided. It is true that in regions to which we are unable to extend experimental research such processes may take place; nay, we may say that certainly there occur in nature many kinds of action which are unlike any we are familiar with. But it is one thing to recognise such things as possible or probable, another to accept them as legitimate explanations of observed facts. Now, processes resembling vegetation, recurring within a period of a few days, taking place in an atmosphere more tenuous than the vacuum of a receiver, repeated after a fortnight of intense cold, and brought about by as remarkable an intensity of heat, must be regarded as quite beyond our experience, and therefore affording a very unsafe basis for reasoning, to say the least. Nor is it a fact unworthy of being noticed that the apparent maximum effect on the Floor of Plato occurs at the noon hour of the place instead of at the hour corresponding to two o'clock in the afternoon, when, according to our experience, the accumulated effect of solar action is greatest. Then, again, if we compare the darkening of Plato to some of those chemical processes with which we are familiar as effects of solar action, we find the change after lunar noon at Plato unintelligible, since assuredly any chemical process progressing as day advanced should not come to an end at noon and then be replaced by the reverse process, but would continue until the evening hours, and according to our ordinary terrestrial experience would leave a permanent effect.

Setting aside other possible explanations—for it is a mistake to suppose, as Mr. Birt appears to do, that surfaces of different tints must maintain the same relative tints under varying illumination—there is the effect of contrast to be considered. This I believe, from my own observations, to afford the true explanation of the observed phenomena. Plato lies on lunar highlands, which shine very brilliantly under high solar illumination. Towards the Mare Imbrium a comparatively narrow ridge separates the floor from that region. Now when the terminator has just passed beyond Plato, the surrounding wall is not nearly so bright as at the time of full moon; the black shadow of its western ridge occupies the western side of the floor; and the eye, in estimating the tint of Plato, is neither oppressed with the glare of general light on the one hand, nor forced to compare the tint of the floor directly and solely with a much brighter surface; if the comparison is made on the east, with an illuminated wall, it is made on the west with a perfectly black shadow-streak. Similar remarks apply to the time when the terminator is about to pass away from Plato. But at the time of full moon, the highlands around Plato are very brightly illuminated. The glare necessarily makes Plato itself look relatively dark, notwithstanding the fact that the floor is also much more brilliantly illuminated; for it is a recognised fact, that surfaces of unequal light-reflecting capacity appear to differ more in brightness under a high illumination than when they are only faintly illuminated. We know, in fact, that a surface which is only dark looks almost or perfectly black when itself and a bright background are under strong illumination.

We have recently had some remarkable illustrations of the deceptive effects of contrast in the aspect of Jupiter's fourth satellite as it has crossed the face of the planet. This satellite is somewhat inferior in light-reflecting capacity to the other three satellites, and if it were not for the physiological law into which I am now enquiring, we should expect this satellite to look somewhat darker than the others when transiting the disc of Jupiter. But as a matter of fact, instead of looking merely dark, this satellite looks nearly black when on Jupiter's face, insomuch that it has been often mistaken for a shadow of a satellite. Mr. Roberts, when observing one of those dark transits of the fourth satellite, could scarcely believe that the satellite would be visible at all when outside the disc; and yet, on every such occasion, as soon as the transit was over, the satellite was seen as usual, though, when close to the planet, looking rather faint by contrast. Mr. N. E. Green, a very excellent observer, and to whom we owe some admirable pictures of Mars and Jupiter, made the following even more pertinent observation on the 26th of March last:—"The transit of the fourth satellite," he says "was closely observed; it certainly appeared as dark as any of the shadows, sometimes even sooty in its blackness, and on leaving the disk seemed unusually faint. But here a remarkable fact, in connection with the law of contrasts, was observed. No sooner had it passed away into the clear sky than it seemed to be brighter than the dark belt against which it had so recently appeared as a decided dark." Such an observation as this appears to me to be decisive against mere eye-estimations, showing that absolutely no reliance can be placed on them unless some contrivance is employed to destroy the effect of contrast. As it is conceded that none of the observed darkenings of the Floor of Plato have been other than eye-estimates, no further discussion seems needed, or can in my opinion be legitimately given to the subject. I may mention, however, that having, though as yet in an imperfect manner, studied Plato with a much reduced field, so that I could eliminate to some degree the effects of contrast,* I have not found that the floor grows relatively darker towards the time of full moon.

The other circumstance which has been referred to change, the variation of the visibility of the spots on the Floor of Plato, cannot be explained as due to contrast, since, in point of fact, the study of different spots introduces a correction for any effects of contrast. Contrast might make any given spot more conspicuous at one time than at another; but it could not cause one spot to be visible at one time when another was invisible, and then after a time the latter to be visible when the former was unseen. As this is what has been detected in the spots on the Floor of Plato, we must either believe that a real change takes place in these spots, or else we must adopt some optical explanation other than that depending on the laws of contrast.

There is a difficulty in the assumption of real change in these spots which does not present itself when we are considering the change of a single crater, or of the floor regarded as a whole. It is not easy to imagine any processes by which different spots in the same limited region of the moon, and under like circumstances, should be very differently affected. If these spots were vegetation-covered, we should expect them to show similar variations; or if we admitted the possibility of different effects, resulting from the same general process, we should still require evidence showing that the changes of visibility had as a period either the lunar day or the lunar year, neither of which relations has in any single instance appeared; and similar results follow whatever real changes we imagine to have affected these regions.

But if we enquire into apparent changes taking place, we find a case precisely corresponding to that of Linné. In Linné the white spot assumes varying proportions and dimensions with varying illumination, and doubtless also with varying position as libration operates to shift the region. Now the aspect of the Floor of Plato, as seen with a powerful telescope, suggests precisely that condition of the surface which would render its appearance most likely to be affected by such changes. The floor is in a general sense level, though it does not suggest the idea of smoothness. It looks as if it were granulated, and the streaks and spots present the appearance of having a surface not differing in tint alone, but in texture; or rather (such at least as been my own opinion when I have studied the Floor of Plato with Lord Lindsay's 12¼-inch telescope) they suggest the impression that their difference of tone is due much more to difference of surface-texture than to difference of tint. Now we view the Floor of Plato very obliquely, the line of sight having a mean inclination of only 40° to the surface, and libration therefore affects the direction of vision very importantly, according to the principle indicated above. The range, in fact, roughly is, from an angle of 33° to one of 47°, or the greatest and least angles are nearly as 3 to 2. It would not be at all wonderful, therefore, if a surface such as the Floor of Plato varied greatly in appearance—now one, now another part being the darker.

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* In point of fact, the field was larger than Plato; but I was able to get the boundary of the field across the floor, and thus to produce effects corresponding to those at sunrise and sunset, when there is shadow on the western or eastern side of the crater.

The following experiment should be tried by those who imagine that the unequal affections of such spots as are on the Floor of Plato manifestly indicate real change. Let a flat circular tin be filled with sandy earth, and over parts of this earth let drops of different liquids be let fall, some colourless, some slightly tinted, some drying with a glazed surface, and so on. Let also certain spots be formed on the surface, by removing portions of the sandy earth and substituting finely crushed glass of various neutral tints. Now let the general surface be viewed at an angle of about 40° (that is, the line of sight inclined 50° to the normal to the surface); then (1) let the direction of illumination be varied, while the direction of sight remains unchanged, and (2) let the direction of the line of sight be changed through 6° or 7° on either side of the original angle of 40°. I venture to affirm very confidently that the behaviour of some of the spots will satisfactorily prove that apparent changes of relative brightness are no sufficient evidence of real changes in the nature or condition of a surface.

The mistake seems to me also to have been made of supposing that, because a lunar surface looks smooth, or because the terminator when crossing the surface shows no indentations, therefore the conditions of illumination may be discussed as though the surface really were optically smooth. In point of fact, if a surface were covered over with minute cones a quarter of an inch in height, it would present the same peculiarities of general illumination as though it were covered with conical hills several hundred feet in height. Now it may be said that as no such irregularities, whether large or small, can be detected in the Floor of Plato, it is not admissible to make the possible existence of such peculiarities a basis of reasoning. That is perfectly just; but it is equally inadmissible to make the possible smoothness of the Floor of Plato a basis of reasoning. We have no direct evidence either way. As to probabilities, it seems at least as likely that the floor is covered with minute elevations as that it is smooth; *nay*, if we remember that the floors of the lunar craters present all the appearance of having once been fluid with intensity of heat, it seems more reasonable to infer that their surfaces now have a crystalline structure than that they are smooth.

We may, indeed, sum up the evidence obtained by Mr. Birt in this way:—It implies either real changes, or surface peculiarities, probably of the nature of minute irregularities, such as result from crystallisation. If real changes be regarded as very unlikely, we have strong probable evidence in favour of surface peculiarities, a result of considerable interest. If surface irregularities be thought very improbable, then we have strong evidence in favour of real changes, a result also of considerable interest. Whether it is more unlikely that real changes so affect these spots as to make their whole surface change in brightness (though no large surfaces ever show such changes), or that the once fluid surfaces within the craters should have a granulated surface different in different parts, is a question which will be answered according to the general views of the lunar student. I have no hesitation in adopting the second view as far the more probable.

I would not, however, have it understood by any means that I think it unlikely that change is taking place on the moon's surface. On the contrary, when one considers the wide variations of temperature to which the surface of our satellite is exposed, it seems altogether probable that (1) a process of disintegration must be in progress, which should at times, one could suppose, produce even remarkable catastrophes on the moon's surface; and (2) that the lunar atmosphere, tenuous though it undoubtedly is, may be affected by changes of condition detectible under certain conditions from our distant standpoint.

Still less would I have it thought that such researches as those which Mr. Birt has advocated, and to some degree prosecuted, should be discontinued as useless. On the contrary, I consider in the first place that they have already led to results which, however interpreted, are of great interest and importance, and that in the second they are the only possible means for arriving at a solution of the problems suggested by the general aspect of our satellite.

What I wish, however, to urge earnestly on students of the moon is the necessity of perfect independence of preconceived opinions as they proceed in their enquiries. The results they obtain should not be held to owe their importance to the evidence they seem to give in favour of this, that, or the other theory, but should be discussed altogether without bias for one view or another. If in this essay I have given prominence to the objections which may be urged against the evidence thus far obtained, so far at least as it has been regarded as evidence of real change, it is solely because, in my judgment, this is necessary to correct a contrary tendency on the part of those who consider that real changes have been demonstrated. I shall venture to quote, in conclusion, the words of a well-known student of the moon, who, if he inclines somewhat (as his words will show) to the theory that change has taken place, nevertheless presents very fairly the doubts which really surround the whole subject. Thus, then, speaks the Rev. Mr. Webb, after describing some of the principal causes of deception by which changes of illumination, or of direction of vision may affect the aspect of lunar regions:—

" It would be easy to extend this list of causes of deception; but those here given may suffice as indications of the caution with which it is necessary to approach the much disputed question of still existing physical change. In the answer to that question—the affirmative answer—undoubtedly lies a great part of the charm of selenography. Whatever may be the magnificence of the abrupter features of the lunar scenery, or the smooth and tranquil aspect of its gentler valleys and wide-extended plains, we shall contemplate them with a different amount of interest accordingly as we are obliged to consider them an inanimate and silent record of the worn-out and spent convulsions of bygone ages, and forces wholly extinct in selenological death; or whether we may detect if it be but the last feeble efforts of that marvellous working which once threw open such amazing gulfs, and piled up such terraces and towers and pyramids, and overspread such wide-extended areas of the globe with confusion and ruin. Some observers may have perhaps been precipitate in assuming the utter and final collapse of all those ancient and evidently long-enduring energies. It were safer to wait and see whether all is indeed so dead and cold. And again, we must not assume, we have to prove—if it can be proved—the absence of atmospheric phenomena. This is not one of those cases where an undemonstrated negative may suffice. The burden of proof—or rather disproof—here naturally rests upon the opponent, when all analogy is in favour of some kind of gaseous envelope: and whatever may, or rather must, be its tenuity as compared with our own, its total absence would be contrary to all chemical and mechanical probability. Nor is it a mere theoretical question: indications are not wanting that the inferences of Schröter and Gruithuisen, to whatever exception they may be liable in their full extent, are at any rate deserving of some consideration. We may be called upon to make abundant deductions on the score of precipitancy and prepossession, and yet a residuum may be found to exist, small in amount, but refractory in character, which cannot be disposed of by any summary mode of treatment. Simple negation will not suffice, much less contemptuous neglect of the labours of those who have preceded us. The first general aspect of that great world lying in its confusion and desolation may indeed be, to some eyes, that of absolute quiescence and arid sterility; a wilderness of rock and sand, lifeless and even soundless, in its unclothed contact with the emptiness of boundless space. But the student, in proportion to his earnestness and perseverance, may see cause to be distrustful of first impressions; he will rather be looking out carefully for those minute indications—and experience has proved that only minute ones can be expected—which may yet show to a well-trained eye and cautious judgment that such a conclusion would be too precipitate. At any rate the question is not yet set at rest; and it can only be finally decided by the faithful carrying out, in very circumstantial detail and with scrupulous accuracy, of the graphical representation of the moon's surface."

NOTICES OF BOOKS.

THE PYRAMIDS OF EGYPT.*

A NEW work on the Pyramids of Egypt has been produced in the present year in France, by M. Dufeu, Member of the Egyptian Institute, and of the Society of Historical Studies of Paris.

It is a goodly-sized octavo, of 322 pages, and claims to have discovered the true object, end, and aim of the "Four Pyramids of Gizeh;" by means of methods, too, and strange discoveries, which are entirely new to the world: while the chief results thus attained to, are that—

(1). Menes ascended the throne of Egypt at the date of 5641 B.C.
(2). The Great Pyramid was founded in 4862 B.C.
(3). All the Pyramids are scientific monuments, and scientific only; and—
(4). The authors of the designs of all of them were the priests of the profane idolatrous Egyptian religion.

The names of Champollion, De Rougé, Mariette, Lepsius, and other well known scholars and *literati* occur so frequently through the pages of this dazzling book, that one might at first expect the author to be of the school of the modern Egyptologists; but that is far from being the case, for there is no deciphering of any piece of hieroglyphics all through the work; and its chief method of proceeding is the wonderful assertion that the list of Manetho, so long held to be a chronological and historical account of the kings of Egypt, is in reality nothing but a series of readings (in an arbitrary unit) on the scale of the Nilometer near Cairo, of the successive annual inundations of the Nile, combined with occasional geological variations of the level of the land through which it flows: a wild theory which no Egyptologist has yet been found venturesome enough to take up with.

Again, there are so many quotations of figures representing either cubits, metres, feet, or inches, and so much assertion of science existing in the Pyramids,—that some persons might imagine that the author is a partaker in that particular "scientific theory," which was commenced by the late John Taylor, in London, and has been carried on since his time to farther developments by many other workers, including myself. But that is also far from being the case: for M. Dufeu's results in chronology, science, and religion are perfectly different from those of the English scientific and *sacred* theory; and his methods of proceeding are distinguished far more by ignorance than knowledge, of the absolute local and monumental facts.

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* Découverte de l'Age et de la veritable destination des quatre Pyramides de Gizeh, Principalement de la Grande Pyramide. Par A. DUFEU. Paris: Ve A. Morel et Cie., 1873.

The very title of M. Dufeu's book is a senseless antagonism to Pyramidal measured and known data, when he therein speaks of "the *four* Pyramids of Gizeh;" for who, after once seeing the Pyramids of Gizeh (Jeezeh), would ever think of speaking of them as *four*, when there are three large ones and two sets of three small ones each, or nine altogether? *One*, he might speak of, because one of them is larger and better than all the rest; or *two*, he might name, because the second one is so nearly as high as the first that many old Arab authors alluded to them as "the pair;" and *three* he might talk of because the third, though by no means so large as either the first or the second, is yet far larger than all the others, stands in a line with the first and second, and is thought by some authors to have been even more expensive than them in construction, on account of the large quantity of granite employed in its casing, making it through all history "the coloured Pyramid." But the moment any one passes beyond this third one, he must, to be truthful, either mention the six small ones, little mites of things though they be, or none at all.

And then behold the "science" of the book! M. Dufeu asserts that the Great Pyramid commemorates its own longitude! But how, and from what, and why?

By taking the profane cubit of Egypt, dividing it into 360 little parts, and representing in those previously unheard of and unused terms, an unimportant feature of the unfinished, subterranean chamber of the Great Pyramid, adding, multiplying, and dividing by other arbitrary numbers representing the rise of some Nile inundations at some time or other, the author at last gets a number which, he says, enables him to state that the longitude of the Great Pyramid was 152° 38′ 20″ from a certain point in the 44th degree of latitude; but whether that latitude was in the Northern or Southern Hemisphere, and whether the meridian was east or west of the Great Pyramid, he finds nothing to define. So there are four points to choose amongst; and as three of them fall in deep ocean water, but the fourth happily alights on land in the American continent, in the Oregon district—the author adopts that point, and exclaims "*therefore* America had been discovered and known by them (the Pyramid builders) before the foundation of the Great Pyramid of Gizeh, 6735 years ago" (see page 210). Yet the excellent M. Dufeu never seems to have considered that if the longitude of the Great Pyramid was measured from a point in America and not that point in America from the Great Pyramid, if there was any longitude measuring at all—America should have been a more civilised, advanced, and developed country than Egypt, in that very early day, and have left behind it some marvellous trophies of science and art, philosophy, history, and literature, both sacred and profane, dwarfing utterly in their age all the remains that have come down to us from all the "five great empires of the East."

Next, let us take an example of ignorance of Pyramid surface facts of a very important class. In his chapter 14, the author arrives at the conclusion, that "the Great Pyramid was never cased," or coated with the much talked of sheet of smooth, bevilled casing stones; and because, he says, there is not only no such casing now to be seen, but no fragments even of it; none of the prismatic edges or corners of the stones, which would have been knocked off at the place, if the casing stones had been pulled down by the early Caliphs, and carried away to build Cairo, as Arab tradition reports was done. Of such fragments M. Dufeu has the hardihood to declare—

> *On n'apercoit pas la moindre indication, la moindre trace de débris de nature a révéler un pareil travail;*—

and again—

> *rien de cela ne s'apercoit.*

Yet I, having carefully examined in 1865 the four enormous heaps of rubbish on the four sides of the Great Pyramid, have found them almost entirely made up of fragments of the peculiarly white Mokattam limestone employed for the casing stones; and have further been rewarded by finding many of the "prismatic" corners and edges of casing stones, and nothing but casing stones, giving under measurement the very characteristic and crucial angle of the slope of the sides of the Great Pyramid. A collection of these angular fragments I had the honour of presenting, in 1867, to the Royal Society of Edinburgh, where they may be seen in a special case in their Museum: while only last year the interesting present was made to me, by my friend Mr. Waynman Dixon, C.E., who was then at the Great Pyramid, of an almost complete casing stone, which he had found amongst the rubbish on the north side of the Great Pyramid's base, together with large fragments of several others. Mr. W. Dixon's completer specimen measures 25 inches long, 21 high, 30 thick, has the outer bevilled slope of 51°, 51′±, and is now in the official residence of the Astronomer Royal for Scotland: a solid witness to the absolute folly of M. Dufeu's assertion, and a proof of his utter ignorance of the most essential facts connected with the exterior of the Great Pyramid.

But now for more curious things touching the interior.

In chapter 17, the author discusses the coffer or sarcophagus, in the king's chamber inside the Great Pyramid. In so doing, he speaks of it as "a box;" considers it to be a representation, by means of outside and inside measures, arbitrarily multiplied and divided by him, merely of the length of the profane

Egyptian cubit; in illustration whereof he gives, in his table 5, five different sets of measures of the said monolithic stone box, coffer, or sarcophagus.

Of these sets of measures, the first one is intituled as being "by the computation of Piazzi Smith and Joseph Jopling."

Now, if the first name is intended for mine, I have to say that Joseph Jopling is now dead, and I never saw him during life, and never worked in concert with him in anything; and though I have read some of his writings, I never approved of his hypothetical ideas about the size of the coffer, but, on the contrary, maintained that they were very erroneous. What I have, at any time, published for the size of the coffer as my own results were the means of very numerous measures taken by myself at the place—more numerous, indeed, than all the published measures yet taken by anyone and everyone in modern times (see my "Life and Work at the Great Pyramid," vol. ii.) and very different in result or amount from the numbers attributed to me by M. Dufeu, with what object I do not pretend to know.

The fourth column of measures set forth by M. Dufeu is labelled as being by Professor Greaves, the Oxford Astronomer, whom the author seems to consider a very late authority on the Pyramid, and a great deal more trustworthy than me; yet he died two hundred and thirty years ago, and all his measures were taken merely at a single visit on one particular day to the Pyramid, with a janissary guarding the entrance all the time.

The fifth set of measures M. Dufeu entitles "notre mesure;" and yet, such title notwithstanding, I am compelled to dispute that the numbers which he gives could really have been measured upon the coffer of the king's chamber in the Great Pyramid, by any "savant" whatever.

My special reasons for thus declining to accept the statement of a Member of the Egyptian Institute, are, that if the gentleman himself, or any friend of his, had really measured each of the six elements of the coffer, as he has recorded them, to 0·001 of an inch, he could not have failed to have discovered that one side at one part of its height was longer than the other by a whole inch; that three of the sides are curved and not flat; and that there is a *quasi* sarcophagus ledge of large size cut out of the substance of the top of all the four sides, raising very serious questions as to how the measures are to be taken; yet not one word on any of these most noteworthy features is there throughout the whole book.

In conclusion, though the author is on every few pages declaring that his theory is entirely new, and that he is therefore a hero to publish it, I am sorry to say that it is not more new than it is true; for the greater part of it was invented, written, and printed for private circulation in 1863, by Hekekyan Bey, an old Armenian officer now resident in Cairo; was discussed with him there by me in 1864, and noticed as untenable in my "Life and Work" book published in 1867.

M. Dufeu has indeed added something to Hekekyan Bey's original matter, such as the "Longitude of the Great Pyramid," "the proof that it was never cased," and his own alleged "coffer measures,"—but they are precisely the most flagrantly erroneous parts of the whole book.

PIAZZI SMYTH,
Astronomer Royal for Scotland.

The Moon: her Motions, Aspect, Scenery, and Physical Conditions. By RICHARD A. PROCTOR, B.A. With three Lunar Photographs by Rutherford, and many Plates, Charts, &c. London: Longmans, Green, and Co. 1873. 8vo. 394 pp.

THE admirable works on Astronomy written by Mr. Proctor during the last few years possess many features which make them peculiarly acceptable to this period—a period marked in the history of Science as one in which a general desire is manifested by all classes to obtain accurate, and at the same time popular, knowledge of the universe. These works are eminently popular; they are pleasantly written, the abstruse treatment of difficult subjects is avoided, and the illustrations are abundant and novel. The author aims specially at original treatment; he does not merely give the reader a compilation, after the manner of the generality of popular scientific writers, but he introduces extended descriptions of various phenomena, and frequently supplies information existing only in the memoirs of some learned Society, or not to be found elsewhere. This was no less noticeable in the last book of Mr. Proctor's ("The Sun") which we reviewed in this Journal, as in the present. Between the appearance of these works less than four years have intervened, yet in this time our author has published some three or four works which have taken their place among the popular scientific literature of the day, and which bear upon them the stamp of much earnest and accurate work.

The work before us is divided into six chapters, which treat respectively of—(1) The Moon's Distance, Size, and Mass; (2) The Moon's Motions; (3) The Moon's Changes of Aspect, Rotation, Libration, &c.; (4) Study of the Moon's Surface; (5) Lunar Celestial Phenomena; (6) Condition of the Moon's Surface. These are followed by Tables, and an Index to the Map of the Moon.

The first chapter opens with an account of the view of the Ancients regarding the moon, and the mode of measuring her distance from the earth. From this we learn that Aristarchus of Samos calculated the distance of the moon (by an unknown method) at two millions of stadia, or about 230,000 miles, while the estimates given by Ptolemy and Alfonso X. approach 235,000 miles. Tycho Brahe calculated the distance as 223,000 miles. The real distance, deduced by Professor Adams from the observations of Breen at the Cape of Good Hope, would appear to be 238,818 miles. We may roughly take the moon's diameter as two-sevenths that of the earth, the moon's surface as twenty-two-sevenths, and her mass at two-ninety-ninths. The surface would be about equal to that of Europe and Africa together, or of North and South America taken together. The earth's disc, as seen from the moon, would appear to be 13½ times larger than the moon appears to us.

The second chapter, which treats of the moon's motions, is one of the most important in the book. It discusses the subject in many points more fully either than Sir G. B. Airy (in his article "Gravitation") or Sir John Herschel (in his "Outlines of Astronomy"). This chapter occupies nearly one-fourth of the entire work, and it is fully illustrated by designs of the author; among others, illustrations of the advance of the perigee and the retreat of the nodes. It is altogether an elaborate exposition of a most difficult subject, which has engaged the attention of the most eminent astronomers and mathematicians of the last century and a half. Our author says, in conclusion, "In the whole history of the researches by which men have endeavoured to master the secrets of Nature, no chapter is more encouraging than that which relates to the interpretation of the lunar motions."

In the account of the study of the moon's surface we have some interesting details concerning the colour of the moon. As to the general results of telescopic observation of the surface, we have to remember the circumstances under which they are made and the power applied. "The highest power yet applied to the moon (a power of about six thousand) brings her, so to speak, to a distance of 40 miles—a distance far too great for objects of moderate size to become visible. Many of my readers have probably seen Mont Blanc from the neighbourhood of Geneva, a distance of about 40 miles. At this distance the proportions of vast snow-covered hills and rocks are dwarfed almost to nothingness, extensive glaciers are quite imperceptible, and any attempt to recognise the presence of living creatures or of their dwellings (with the unaided eye) is utterly useless." . . . Again, as to other difficulties, "We view celestial objects through tubes placed at the bottom of a vast aërial ocean, never at rest through any portion of its depth; and the atmospheric undulations which even the naked eye is able to detect are magnified just in proportion to the power employed. These undulations are the bane of the telescopist. What could be done with telescopes, if it were not for these obstructions to perfect vision, may be gathered from the results of Professor Smyth's observations from the summit of Teneriffe. Raised above the densest and most disturbed strata, he found the powers of his telescope increased to a marvellous extent. Stars which he had looked for in vain with the same instrument, in Edinburgh, now shone with admirable distinctness and brilliancy. Three delicate striplings of the discs of Jupiter and Saturn, which require in England the powers of the largest telescopes, were clearly seen in the excellent but small telescope he employed in his researches. It is probably not too much to say that even if the Rosse telescope were perfect in defining power, which unfortunately is very far indeed from being the case, yet, on account of atmospheric disturbance, instead of reducing the moon's distance to 40 miles, it would in fact not be really effective enough to reduce that distance to less than 150 miles." Remembering all this, we see that we must receive with great caution observations as to the colour of the moon, the texture of its surface, and so on; the apparently smooth seas or sea-bottoms may in reality be hilly and irregular. The most notable feature of the moon's surface is perhaps the crateriform mountains, which Webb has divided into "walled or bulwarked plains, ring mountains, craters, and saucer-shaped depressions or pits. . . . Copernicus is one of the grandest craters, 56 miles in diameter. It has a central mountain (2400 feet in height, according to Schmidt), two of whose six heads are conspicuous; and a noble ring composed not only of terraces, but distinct heights separated by ravines; the summit, a narrow ridge, not quite insular, rises 11,000 feet above the bottom, the height of Etna, after which Hevel named it. Schmidt gives it nearly 12,800 feet, with a peak of 13,500 feet west, and an inclination in some places of 60°."

Many attempts have been made to determine whether the moon be inhabited or not. Herschel held the former opinion, and many expected that Herschel's or Rosse's great reflector would reveal something of the inhabitants. But in vain. Of course the creatures themselves could not be visible, but large cities might appear; and it has been argued that, as the force of gravity is so much less at the surface of the moon than on the earth, the lunar inhabitants might, without being unwieldy, be much larger than our race of men. "Nor is this argument wholly fanciful. A man of average strength and agility placed on the lunar surface (and supposed to preserve his usual powers under the somewhat inconvenient circumstances in which he would there find himself) could easily spring four or five times his own height, and could lift with ease a mass which, on the earth, would weigh half a ton. Thus it would not only be possible for races of lunarians equal in strength to terrestrial races to erect buildings much larger than those erected by man, but it would be *necessary* to the stability of lunar dwellings that they should be built on a massive and stupendous scale. Further, it would be convenient that the lunarians, by increased dimensions and more solid proportions, should lose a portion of the superabundant agility above indicated." It only remains to be mentioned that no object which could possibly be erected by an intelligent being has ever been observed on the moon's surface, even by the use of our most powerful telescopes. Neither, during the two centuries and a half in which the moon has been carefully scrutinised, has any evidence of physical change appeared; all evidence seems, indeed, to show that the moon is "a dead and useless waste of extinct volcanoes."

Varieties of colour are noticeable on the moon's surface; some regions appear white, and would be spoken of as *snow-covered* if it were not impossible from the fact that water and air do not exist in the moon. Then there are grey, and greenish, and pale red regions.

An interesting account will be found (pp. 272—282) of the experiments which have been made in order to determine the heat of the moon,—notably the recent experiments of Lord Rosse and M. Marié-Davy. These appear to show that the heat which is received from the moon is mainly radiated, not reflected; that the temperature of the moon's surface is about 500° F.; and that the calorific effect of the full moon is only equal to about one ninety-four-millionth of a degree centigrade. But we must bear in mind that the greater amount of lunar heat which is radiated to the earth is absorbed by the aqueous vapour in our atmosphere.

The fifth chapter discusses, among other things, the possible evidence of a lunar atmosphere. All the known evidence tends to prove that the moon has either no atmosphere at all, or that the atmosphere possesses extreme tenuity. Sir John Herschel, however, and others have admitted the possibility of the existence of an atmosphere on the hemisphere of the moon which is turned away from us, and this theory is based upon the fact that the moon's centre of gravity is nearer to us than her centre of figure.

Among the more striking illustrations in the work are two lunar landscapes, drawn by Mr. Proctor. Of course such landscapes can only faintly indicate what we may imagine an observer would see if he were placed on the surface of the moon. Yet as, our author observes, "we know certain facts,—we know that there are striking forms of irregularity; that the shadows must be much darker, as well during the lunar day as during an earthlit lunar night, than on our own earth in sunlight or moonlight; and we know that whatever features of our own landscapes are certainly due to the action of water, in river, rain, or flood, to the action of wind and weather, or to the growth of forms of vegetation with which we are familiar, ought assuredly not to be shown in any lunar landscape. But a multitude of details absolutely necessary for the due presentation of lunar scenery are absolutely unknown to us. . . . In looking at one of these views (Plates XXI. and XXII.) the observer must suppose himself

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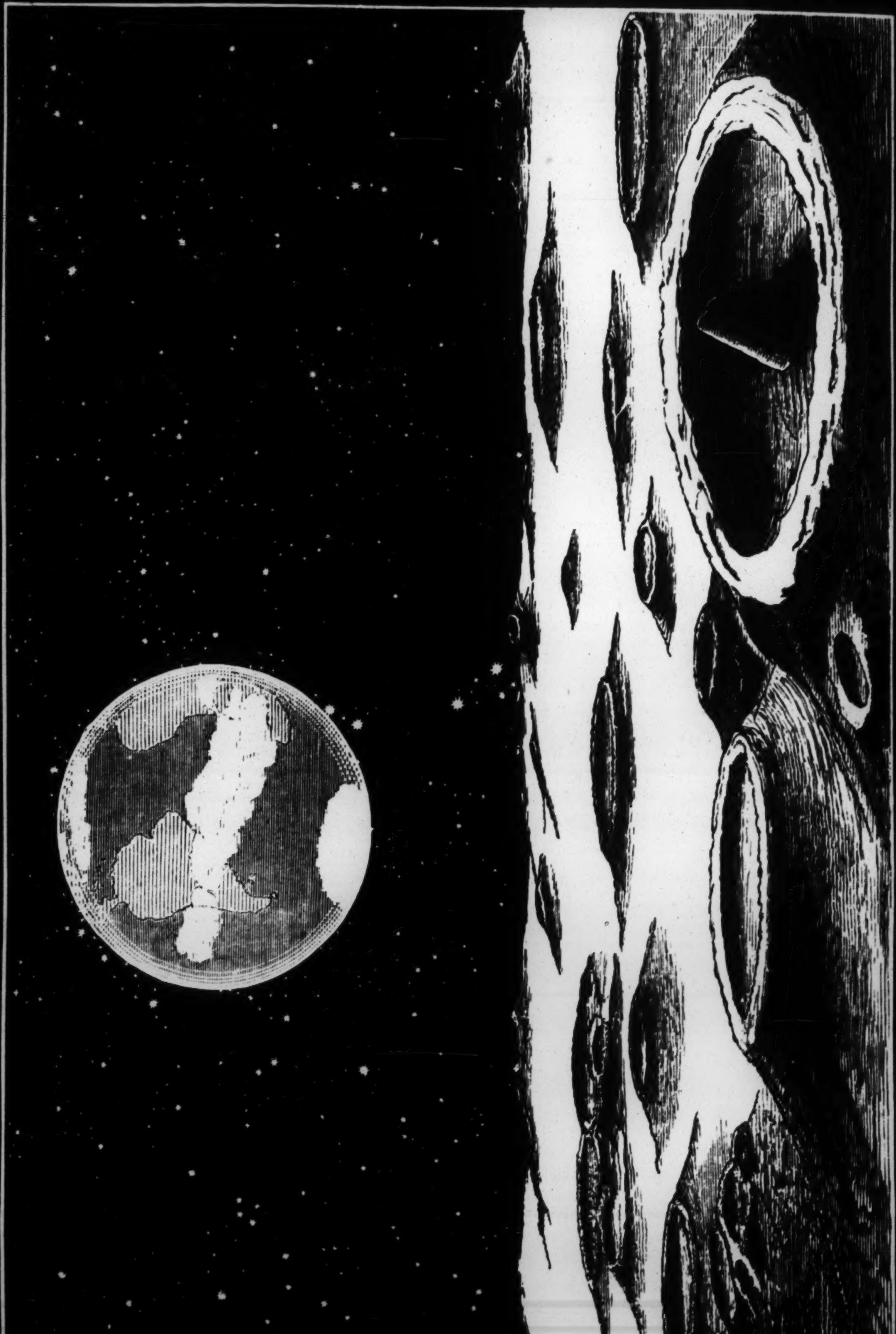
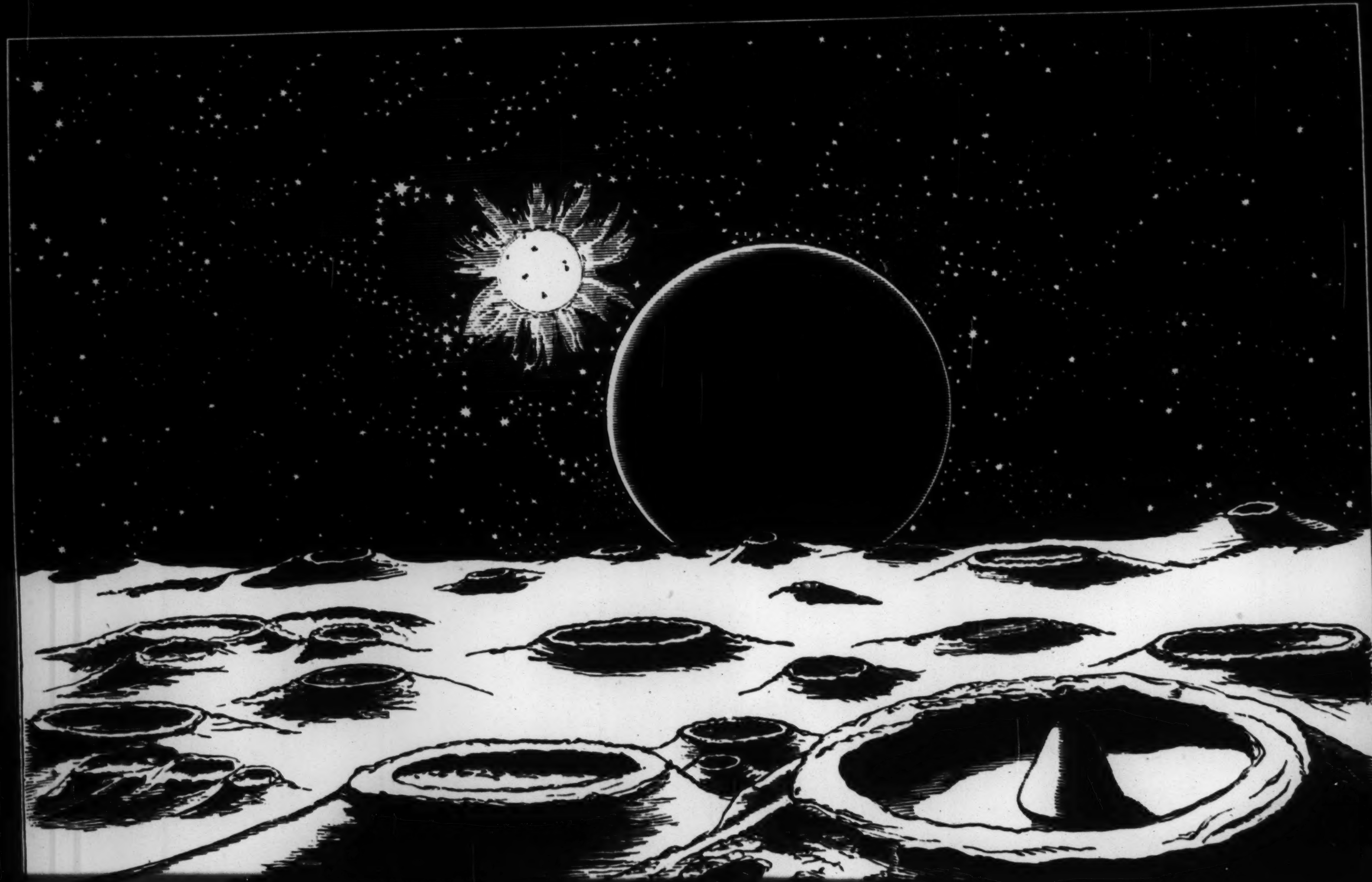
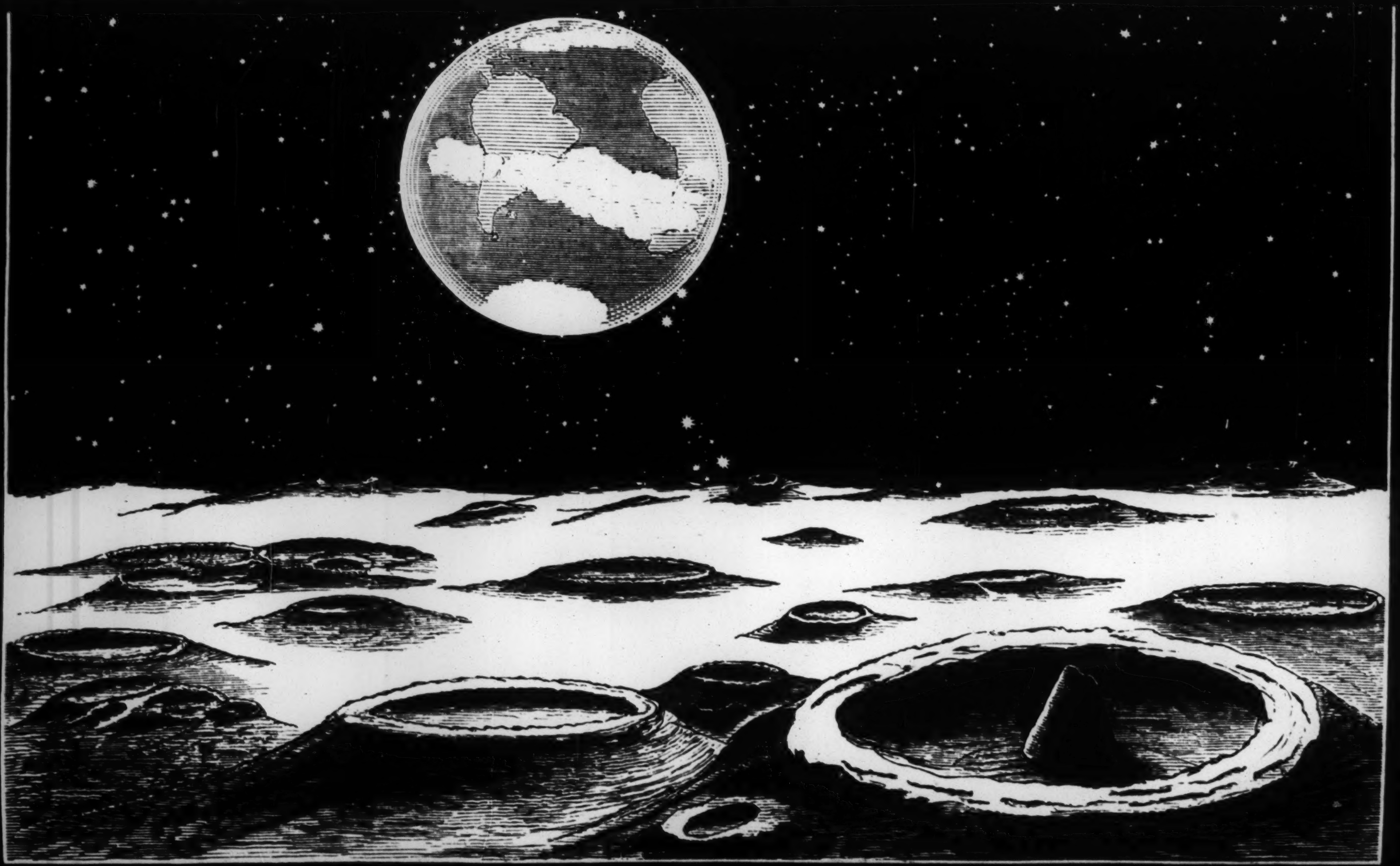
PLATE XXI.



LUNAR LANDSCAPE—"FULL" EARTH.

PLATE XXII.

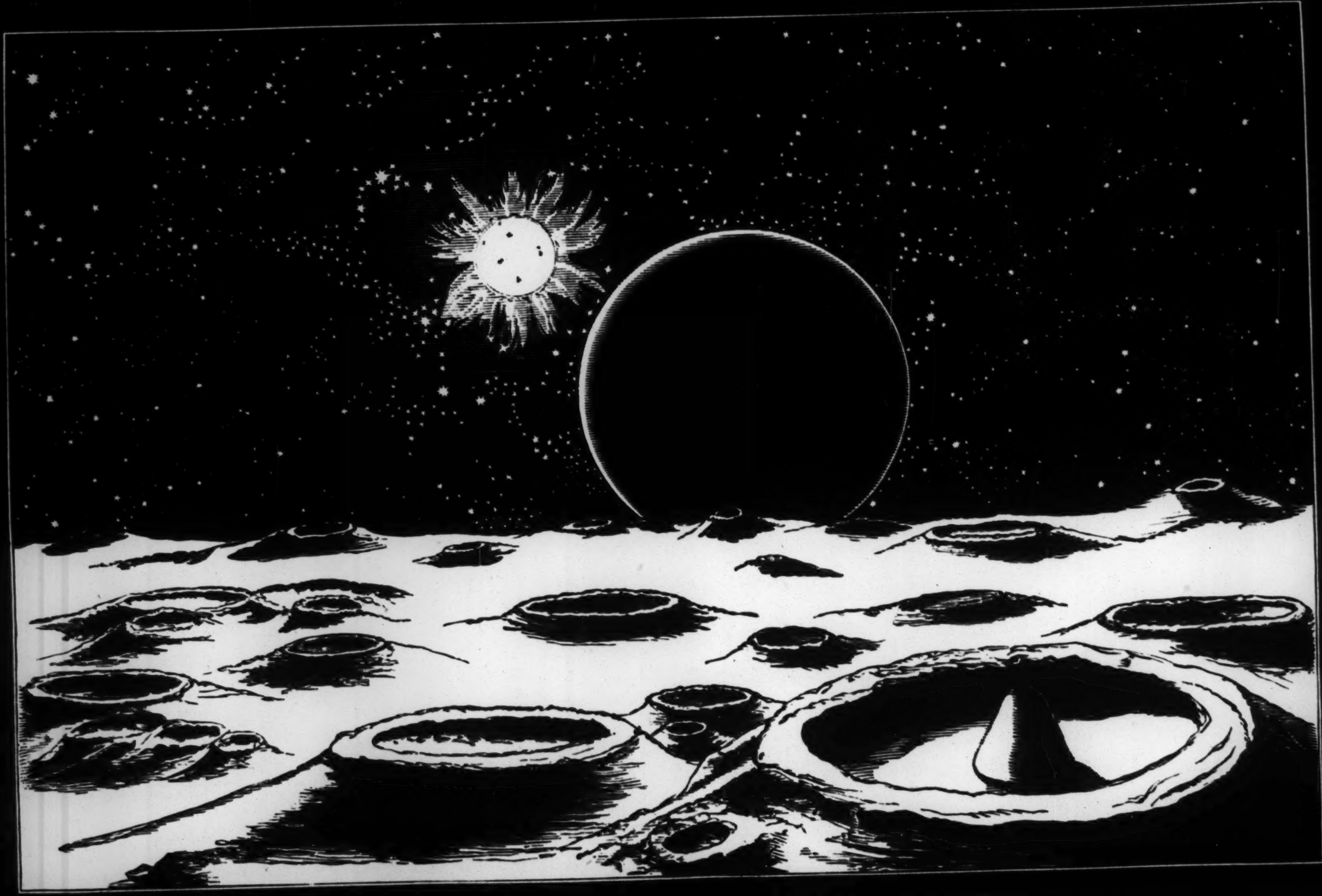




LUNAR LANDSCAPE—"FULL" EARTH.

THE QUARTERLY JOURNAL OF SCIENCE. No. XL., OCT., 1873.

PLATE XXII.



LUNAR LANDSCAPE—"NEW" EARTH.

stationed at the summit of some very lofty peak, and that the view shows only a very small portion of what would really be seen under such circumstances in any particular direction. The portion of the sky shown in either picture extends only a few degrees from the horizon, as is manifest from the dimensions of the earth's disc; and thus it is shown that only a few degrees of the horizon are included in the landscape."

This chapter concludes with a very graphic description of what an observer stationed on the summit of the lunar Apennines would have seen on the evening of November 1, 1872. We recommend all our readers to carefully peruse these most eloquent and beautifully descriptive passages. We may only quote one or two paragraphs as examples:—"On all sides, this mighty star-belt spread its out-lying bands of stars, far away on the one hand towards Lyra and Bootes, where on earth we see no traces of milky lustre, and on the other towards the Twins and the clustering glories of Cancer—the 'dark constellation' of the Ancients, but full of telescopic splendours. Most marvellous, too, appeared the great dark gap which lies between the Milky Way and Taurus; here, in the very heart of the richest region of the heavens, with Orion and the Hyades and Pleiades blazing on one side, and on the other the splendid stream laving the feet of the Twins,—there lay a deep black gulf, which seemed like an opening through our star system into starless depths beyond. . . . And now, as hour passed after hour, a series of changes took place in the scene, which were unlike any that are known to our astronomers on earth. The stars passed, indeed, athwart the heavens on a course not differing from that followed by the stars which illumine our skies, but so slowly that in an hour of lunar time they shifted no more than our stars do in about two minutes. And, marvellous to see, the great orb of earth did not partake in this motion. Hour by hour passed away; the stars slowly moved on their course westwards, but they left the earth still suspended as a vast orb of light high above the southern horizon. She changed, indeed, in aspect. The two Americas passed away towards the right, and the broad Pacific was presented to view. Then Asia and Australia appeared on the left, and as they passed onwards the East Indies came centrally upon the disc. Then the whole breadth of Asia could be recognised, but partly lost in the misty light of the northern half, while the blue of the Indian Ocean was conspicuous in the south. And as the hours passed on, Europe and Africa came into view; and our own England, foreshortened and barely visible, near the snow-covered northern region of the disc."

Here, then, we end our brief examination of a work which commends itself both to the general well-informed reader and to the man of Science. The really new matter is by no means inconsiderable, and the work constitutes, we believe, the most complete monograph on the moon which has yet appeared. We may read every part with a perfect feeling of confidence in the exactness of the matter, remembering that it is at once the work of a mathematician, an astronomer and practical observer, and of an elegant writer.

An Introduction to Physical Measurements. With Appendices on Absolute Electrical Measurement, &c. By Dr. F. KOHLRAUSCH, Professor-in-Ordinary at the Grand Ducal Polytechnic School at Darmstadt, and formerly Professor of Physics at the University of Göttingen. Translated from the second German edition by T. H. WALLER, B.A., B.Sc., and H. R. PROCTER, F.C.S. London: J. and A. Churchill. 1873. 8vo. 244 pp.

SOMEONE (we think Quetelet) has remarked that no science has made any great progress until weight, measure, and number have been introduced into it, in fact, until it has become more or less capable of mathematical treatment. We all know how true this is:—The determination of the mechanical equivalent of heat raised the science to a position which it never before occupied among its brethren; to which result the admirable mathematical deductions of Carnot, Fourier, and, later, of Hirn, Helmholtz, and Clausius have also conduced. Again, Ohm's law, and the various mathematical problems brought to bear upon the subject of electricity by Sir W. Thomson, Clerk Maxwell, and others, have quite revolutionised that science. As an important means to the end indicated above, we welcome the book before us with open hands. It will be invaluable in those physical laboratories which are happily commencing to appear in this country, and which, by the end of the century, will, we trust, have become general in all centres of sound learning.

The author remarks very truly, "that the mere verbal teaching of physical laws is seldom of much use, tending frequently merely to confuse the student, whilst the simple performance of an experiment gives him confidence in himself and in the laws he is investigating." This work on the measurement of physical quantities enables the student the more readily to verify the great laws which obtain in the history of matter and of force.

The introduction treats, in the first place, of "Errors of Observation," and of the influence of error on the final result. And here we meet with advice on a practice which very frequently prevails in the calculation of final results:—"We may here insist upon the fact that it is generally quite inadmissible arbitrarily to exclude from a series of observations some of the number simply because they do not agree with the greater number. The probability of an increased error being introduced by the irregular numbers will be compensated by the very process of taking the arithmetical mean, for, as single ones among a greater number, they have small influence upon the mean value." This is, no doubt, good advice, but very difficult to follow; if, for example, in a series of twenty observations, seventeen agree very closely, and three are altogether anomalous, or at least differ more widely, the conclusion seems to be almost irresistible that, in these three divergent results, the "personal equation" of the observer has, by some unknown means, been unduly exalted, or some unseen or unremembered error of manipulation has crept into the observation. If made absolutely under the same conditions, of course every determination, however anomalous it may be, and however the series may be prolonged, is subject to equal credence.

The first section is devoted to an account of "Weighing and the Determination of Density." In this, full rules are given for the adjustment and testing of a balance and of the weights; also various modes of weighing, and the determination of the density of solids, liquids, and gases. This is followed by measurements relating to heat; the calibration of a thermometer, and determination of its fixed points; the various methods of calorimetry, &c.

The determination of the modulus of elasticity of a body is discussed under various forms, as by stretching, and by the complex and somewhat unusual method by longitudinal vibrations, in which the necessary factors are (α) the length of the wire, (β) its specific gravity, (γ) the acceleration by gravity, (δ) the number of longitudinal vibrations per second. The time of vibration is determined by a tuning-fork of known pitch, and the longitudinal vibrations are produced in the usual manner, by rubbing the rod with woollen cloth sprinkled with resin. The determination of the modulus by bending a rod, and by swinging under torsion, is also described.

The optical measurements include various determinations connected with spectroscopy, the wave-length of a ray of light, the focal length of a lens, the magnifying power of optical instruments, the operations of saccharimetry by polarised light. Finally, a number of magnetic and electrical measurements are described in detail.

The tables at the end of the volume will be found of considerable service; here we find, among others, the density of certain gases given to seven places of decimals, the density of water to five places of decimals between 0° and 30° C., the expansion of water, the density of air at various temperatures, and the capillary depression of mercury in a glass tube of from 1 to 10 millimetres diameter. Table 19 gives the lines of flame-spectra of the most important light metals, according to the scale of Bunsen and Kirchhoff, in which the slit is considered to have the breadth of one division, while the sodium line is taken at 50. Under these conditions, the colours of the spectrum are approximately as follows:—Red to 48, yellow to 52, green to 80, blue to 120, and violet beyond.

A Dictionary of Terms Used in Architecture, Building, Engineering, Mining, Metallurgy, Archæology, the Fine Arts, &c. By JOHN WEALE. Fourth Edition, with Numerous Additions. Edited by ROBERT HUNT, F.R.S. London: Lockwood and Co. 1873.

WHEN a work has so rooted itself in our literature as to reach a fourth edition, the business of a reviewer becomes tolerably simple. Indeed the public has long ago formed its own opinion as to the merits of the work; and the very existence of the successive editions bespeaks its value more clearly than any favourable expressions that may fall from the reviewer's pen.

The rapid growth of modern science, and the consequent extension of scientific terms to the various branches of industry and art, renders it more than ever necessary that we should constantly have at hand some trustworthy work of reference on technical nomenclature. It is difficult to point to a handier book for this purpose than Weale's well-known Dictionary; it is comprehensive, though small, and is, indeed, just the book which one may confidently consult when seeking the interpretation of some obscure word in the language of our industrial arts.

The present edition of Weale's Dictionary has had the benefit of careful revision by Mr. Robert Hunt, whose long experience with kindred literature leads him to know exactly the kind of matter which the public seeks and expects to find in a work of this character. The editor has judiciously adopted, in preparing this edition, a more systematic arrangement of the matter; and, by omitting the biographical sketches which appeared in the earlier editions, has contrived to squeeze in a goodly amount of new matter without increasing the bulk of the book.

A rapid glance down the columns of this Dictionary is sufficient to show that even the best informed amongst us may often turn with profit to its technical definitions. Take, as accidental examples, the first and last words in this Dictionary;—surely it is not everyone who, if suddenly called upon, could give a satisfactory definition of either *Aam* or *Zyghar*.

Celestial Objects of Common Telescopes. By the Rev. T. W. WEBB, M.A., F.R.A.S., Vicar of Hardwick, Herefordshire. London: Longmans, Green, and Co. Third Edition, Revised and Enlarged.

IT is gratifying to learn that such a book as this has reached a third edition. It is not a library book, is by no means likely to be read by Mudie's subscribers, nor purchased by anybody for the mere sake of perusal. It is simply a Guide-Book for travellers in the heavens, one intended and well adapted to the wants of those who have decided to devote some portion of their surplus means to the noblest of human pursuits—the direct study of Nature. We may therefore regard the demand for such a book as a measure of independent astronomical research. We use this word "research" deliberately, and apply it to the humblest efforts of the humblest possessor of the smallest of telescopes who uses such an instrument, or even the naked eye, for the purpose of obtaining knowledge direct from the heavens. We have no sympathy with those scientific prigs who pretend to despise amateur astronomers, and would lead the docile readers of Quarterly Reviews, &c., to believe that a "broad basis of scientific culture" is the exclusive prerogative of University professors and officials. All who have followed the recent progress of Astronomy in this country must be struck with the great amount of scientific work of the highest order that has been done by pure amateurs, by men who have begun with small telescopes and unpretending efforts, and have been led on, by the fascination of the subject, to purchase more and more perfect instruments and aim at higher and higher work, until—in those cases where wealth has accompanied scientific enthusiasm—they have found themselves the proprietors of observatories in which they have made some of the most important of modern astronomical discoveries. The existence of such a body of able amateur astronomers as constitute a large proportion of the Fellows of the Royal Astronomical Society is alike honourable to the nation and advantageous to Science, and we welcome the third edition of Mr. Webb's Handbook as a valuable aid and incitement to valuable and disinterested scientific enthusiasm.

The Depths of the Sea. An Account of the General Results of the Dredging Cruises of H.M.SS. *Porcupine* and *Lightning* during the Summers of 1868, 1869, and 1870, under the Scientific Direction of Dr. Carpenter, F.R.S., J. Gwyn Jeffreys, F.R.S., and Dr. Wyville Thompson, F.R.S. By C. WYVILLE THOMPSON, LL.D., F.R.SS. L. and E., F.L.S., &c., Regius Professor of Natural History in the University of Edinburgh, and Director of the Civilian Scientific Staff of the *Challenger* Expedition. London: Macmillan and Co.

SCIENCE is undoubtedly cosmopolitan; nevertheless the business of scientific research may be materially promoted by a certain amount of international division of labour. If "Britannia rules the waves" she ought to take the lead in studying all that lies beneath and about them. There are solid as well as sentimental reasons for this. We have a huge navy. Our finest ships are liable to rot if left lying idle in dock or harbour, and the best of sailors are subject to analogous corruption unless provided with some kind of active occupation. A genuine sailor has a huge contempt for useless inactivity and lubberly land-lounging, but in these "piping times of peace" he has no small difficulty in finding some plausible pretext for a cruise. Pirates are practically extinct; there is nothing to be done in chasing them; the only approach to old-fashioned naval occupation now remaining open to him is the weary blockading of the pestiferous mouths of swampy African rivers for the meagre chance of occasionally capturing a slaver.

The *Times* newspaper has recently taken up the subject of Arctic Exploration, and would have us give up all further attempts to solve the polar problems, because the private and imperfectly-organised expedition of the *Polaris* has compelled some sailors and Esquimaux to suffer the hardship of wintering on a drifting ice-floe. If sailors were helpless babes and the *Times* were the national wet-nurse for marine infants, this tender solicitude would be emphatically proper and dutiful, but, as it is, the opinions of the quarter-deck and forecastle are far better guides for outsiders, like ourselves, than those of Printing-House Square. If responsible officers and sober men, who have already had some practical experience of arctic hardships and dangers, are willing and eager to incur them again, and if full-grown civilian naturalists are equally urgent in their desire to share the sailor's perils, it would be little short of insult if the nation at large were to accept the conclusions of the *Times*, and refuse to enter upon further arctic exploration merely on the pretext of maternal tenderness. If the whole truth could be told, we should probably learn that the risks of physical suffering which our sailors encounter in the streets of Valetta, Naples, Genoa, Marseilles, and other Mediterranean ports, while their ships are lying idly in harbour, are quite as great as those to which they are exposed when threading their way between Greenland icebergs.

If any statesman desires to learn how the spare ships and men of the British Navy should be occupied during times of peace, let him send at once to Bedford Street for a copy of Dr. Thompson's "Depths of the Sea;" let him read it thoughtfully, and compare it with the log-book of the ordinary ships of war which we are compelled, at great expense, to maintain in sailing condition for mere preparation sake. He will see, by the continual reference to the hearty co-operation and valuable aid of the naval officers, how readily and aptly the sailor takes to scientific work; and when he reflects on the fact that warfare is becoming more and more a struggle of scientific engineering, the importance of the prevalence of scientific habits of mind among naval officers must be obvious. If he has any old-fashioned patriotism, the perusal of this luxurious volume must stir up a healthy British pride in the truly glorious conquests of the *Porcupine* and the

Lightning during the summers of 1868, '69, and '70, and show him how vast are the conquests that may yet be made in the new and higher career of honour which is opened up for the British flag by such expeditions.

Most of our readers know already, by the reports that have been published from time to time in the scientific and other journals, what was done during these well-spent summers; and we recommend all to revel, as we have done, in the luxury of travelling again over Dr. Thompson's connected and beautifully illustrated narrative of the whole of the proceedings. As a profoundly valuable contribution to science, as a literary effort of high order, and as an elegantly artistic volume, this book is worthy of the warmest praise. It is indeed fortunate that the results of such important expeditions should be recorded by so able a writer as Dr. Thompson, and that such a writer should find such spirited publishers as Messrs. Macmillan and Co.

The simplicity of style and clearness of description are very high merits. The whole book is readable by any man or woman of ordinary liberal education, and this great merit is attained without any sacrifice of scientific technicality or precision. We sincerely hope that the privilege of reading the original record of such important scientific work will be fully and popularly appreciated, as it is not often that researches which have had so important an influence on some of the foundations of cosmical science are thus easily accessible.

The most important philosophical results of the expeditions are summed up in the concluding essay on the "Continuity of the Chalk," wherein the author states his reasons for concluding that, in spite of the myriads or millions of centuries that must have elapsed since the deposit of the chalk which lies beneath our feet here in London, there has been no chasm of time, no interregnum of deposit between this ancient and the actual but somewhat modified chalk formation now proceeding at the bottom of the Atlantic.

Admitting to a certain extent the justice of the objections made by Sir Roderick Murchison and Sir Charles Lyell, to his early expression that "we are still living in the cretaceous epoch," on account of the indefinite sense of the terms "geological epoch" and "geological period," Dr. Thompson shows good reason for maintaining the conclusion which these words were intended to express, viz., that "the various groups of fossils characterising the tertiary beds of Europe and North America represent the constantly altering fauna of the shallower portion of an ocean whose depths are still occupied by a deposit which has been accumulating continuously from the period of the pre-tertiary chalk, and which perpetuates with much modification the pre-tertiary chalk fauna;" or otherwise, that "we must regard the tertiaries as the deposits formed and exposed by depressions and upheavals of the cretaceous sea: of a sea which, with many changes of condition produced by the same oscillations which alternately exposed and submerged the tertiaries, existed continuously, depositing conformable beds of chalk-mud from the period of the ancient chalk."

The important bearing of these conclusions upon the very foundations of geology are obvious enough, and are rendered more strikingly so when expressed in the still more pointed language of Professor Huxley, "that the modern chalk is not only the lineal descendant, so to speak, of the ancient chalk, but that it remains, so to speak, in possession of the ancestral estate; and that from the cretaceous period (if not much earlier) to the present day the deep sea has covered a large part of what is now the area of the Atlantic. But if *Globigerina* and *Terebratula*, *caput serpentis* and *Beryx*, not to mention other forms of animals and plants, thus bridge over the interval between the present and the mezozoic periods, is it possible that the majority of other living things underwent a sea-change into something new and strange all at once?"

Such suggestions are almost revolutionary, and if confirmed they will cruelly spoil the orthodox lecture-room diagrams of the geological ladder and the common stratigraphical descriptions of superposition of rocks in the order of time. All the symmetry of geological chronology will be destroyed if the cretaceous system is to run up through the *eocene*, the *miocene*, the *pliocene*, the *pleistocene*, and the *recent*; and we must cease to call these by the name of "periods," as they may merely indicate localities or variations of sea-depth. If the chronological conclusions based upon the stratigraphical arrangement of these later rocks are thus shaken, may not something analogous have occurred when the lower rocks were forming? May there not be other cases where depths of ocean have been mistaken for depths of time? or, in other words, may not many geological formations hitherto described as deposited successively have actually been proceeding, to some extent, simultaneously?

Thus, again, we are presented with an important phase of the great question of evolution. If the creatures now living in the great depths of the Atlantic can be proved to be the true descendants of those of our chalk cliffs, and their line of ancestry can be traced continuously, we have command of a vast period of time under which to study the laws of modification of varieties, of species, and even perhaps of genera.

But these are problems of vast magnitude, which the cruises of the *Lightning* and the *Porcupine* have only opened or suggested, and which we may hope that the *Challenger* will open yet wider; their complete solution will demand an amount of further research proportionate to their magnitude and great philosophical importance.

We are satisfied that every reader of "The Depths of the Sea" who is earnestly interested in the progress of Science and the true honour of his country will share our comfortable satisfaction in knowing that Dr. Wyville Thompson is the Director of the Civilian Staff of the *Challenger* Expedition; and our conviction that when future historians recite the sea-battles that have been gloriously fought under the British flag, and the names of the ships that have carried it to victory, those of the *Lightning*, the *Porcupine*, and the *Challenger* will take leading rank in the list; and also in our hope that the British Navy is entering upon a new era of higher and better conquests than those which have hitherto fed the pride of the nation.

Critiques and Addresses. By THOMAS HENRY HUXLEY, LL.D., F.R.S. London: Macmillan and Co. 1873.

THIS is a collection of Essays like the "Lay Sermons, which, as Dr. Huxley says, "indicate the high-water mark of the various tides of occupation by which I have been carried along since the beginning of the year 1870. They include the following subjects:—"Administrative Nihilism." "The School Boards: What they Can Do and What they May Do." "Medical Education." "Yeast." "The Formation of Coal." "Coral and Coal Reefs." "The Methods and Results of Ethnology." "Some Fixed Points in British Ethnology." "Palæontology and the Doctrine of Evolution." "Biogenesis and Abiogenesis." "Mr. Darwin's Critics." "The Genealogy of Animals." "Bishop Berkeley on the Metaphysics of Sensation." These titles sufficiently indicate the range of subjects, and the impossibility of including within our limits anything like an analysis or discussion of the contents of this volume. The subjects and their treatment are strikingly characteristic of the noble breadth of Dr. Huxley's attainments and philosophy.

The vast accumulations of modern knowledge have rendered a division of labour among the experts in Science a matter of absolute necessity. We have undoubtedly gained great advantages by one man devoting his life to mathematics, another to metallurgy, a third to organic chemistry, a fourth to comparative anatomy, &c.; it has enabled either of these to learn all that has been done in his particular department, and thus to start fairly upon the path of original investigation. This arrangement is, however, not unaccompanied with disadvantages, some of them rather serious. Among these is the common practice of assuming that Physics, Chemistry, Physiology, Political Economy, Metaphysics, Moral Philosophy, &c., are subjects that have actual separate existences in the scheme of Nature, rather than regarding them in their true aspect as artificial subdivisions of the one and only science of Universal Natural Law, and remembering that such divisions have been made for the accommodation of human weakness. There are but few of our modern teachers of Science that are able fairly to divest themselves of the cramping influences of this patchwork view of Nature. Amongst these few Dr. Huxley stands out with leading prominence. His special study having been that of Biology, which is placed, so to speak, midway between the physical and moral subdivisions of Natural Science, he stands upon a middle eminence, from which he can best survey the equally surrounding area of human knowledge. With an intellectual vision of unusually great penetrating power, and a moral nature of well-balanced proclivities, he is thus able to present to his readers, with remarkable vividness and impartial truthfulness, a picture of the great panorama thus placed at his feet. The "Critiques and Addresses" is a series of these pictures, not avowedly connected and yet not altogether detached. They are all painted with remarkable artistic power, and more or less characterised by a catholicity of treatment which obliterates the artificial boundaries that have been mischievously set up between, physical, physiological, moral, and theological science. The powerful essay on "Mr. Darwin's Critics" is a fine example of this. It is positively refreshing to be able to travel in the midst of a purely scientific atmosphere over a fertile region of thought which is usually rendered pestiferous by the miasma of theological bigotry, and to leave it, as we may after wandering under Professor Huxley's guidance, with a healthily invigorated intellect.

In the first two papers we have political subjects treated in like manner, without at all descending to "the region in which Tories, Whigs, and Radicals 'delight to bark and bite.'"

In the course of his enquiry into the limits of Government functions, Prof. Huxley comes in collision with the conclusions of his friend and fellow-worker—we might almost say twin-brother in Science—Mr. Herbert Spencer, and the consequent combat is conducted with the utmost vigour on both sides, accompanied with a polished courtesy suggestive of a courtly fencing bout between the most chivalric of antagonists. Accepting Mr. Spencer's parallel, Prof. Huxley contends that "the vascular system, or apparatus for distributing commodities in the animal organism, is eminently under the control of the cerebro-spinal nervous centres—a fact which, unless I am again mistaken, is contrary to one of Mr. Spencer's fundamental assumptions. In the animal organism Government does meddle with trade, and even goes so far as to tamper with the currency." We will not venture to step between such combatants, or even to record the "hits" on either side, but merely state in the meantime that few can follow this controversy on a two-sided subject without profiting considerably by seeing both sides so well displayed.

While the subjects usually supposed to belong rather to the provinces of ornamental or controversial literature than to science are thus treated in accordance with strict inductive philosophy, without losing anything of literary refinement or readable simplicity, the other subjects, commonly supposed to be more strictly scientific, are by no means presented in the bare skeleton style of pedantic disquisition which is popularly supposed to be the truly scientific style, but are exhibited to the reader in their truly natural forms, enveloped in the rounded and tinted integuments of a polished literature, which appeals to the emotional—the poetic faculties of the reader, as well as to the purely intellectual powers. In reading these we seem to be listening to a voice that comes genially across a dinner table rather than coldly from a professorial chair.

With these characteristics, we have no doubt this collection of "Critiques and Addresses" will be as popular and usefully influential as the "Lay Sermons" that were published three years ago.

Reprint of Papers on Electro-Statics and Magnetism. By Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.S., F.R.S.E., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow. London: Macmillan and Co. 1872.

A Treatise on Electricity and Magnetism. By JAMES CLERK MAXWELL, M.A., LL.D. Edin., F.R.SS. London and Edinburgh, Honorary Fellow of Trinity College, and Professor of Experimental Physics in the University of Cambridge. 2 vols. (Clarendon Press Series.) London: Macmillan and Co. 1873.

THOSE amongst our readers who are acquainted with periodical scientific literature will find in Sir W. Thomson's "Reprints" much they have already studied and much that will be new to them. But the first idea one obtains from the volume of nearly six hundred pages is the immense amount of unpublished work implied. It is a characteristic of physical science memoirs, and a reason for their scarcity, that they represent not only so much work done upon paper, but long and tedious labours in the laboratory, endless disappointments from having taken the wrong road, and turnings back which have certainly served the purpose of rendering surer the way for future travellers, but wearying in the extreme to the explorer. Another surprise is the practical results that may accrue from purely mathematical deductions, but of which, upon closer inspection, the secret turns out to be the logical and comprehensive argument involved. Such views are encouraging, especially in the science of electricity. We were fast growing into the habit of looking to Germany for our mathematical investigation of electro-statical and electro-magnetical science, and this not because these papers were unknown, but because they were too scattered for immediate reference. Great good has been done by their collection, for they are, to use a German expression, " science-making" in their nature.

We will note in the order of the papers as nearly as possible the chief points of Sir William Thomson's work. The first paper deals with the exceedingly difficult problem of the uniform motion of heat in homogeneous solid bodies and its connection with the mathematical theory of electricity. Before this paper there were prominently current two questions—one resulting from Fourier's investigation, by which it appeared that the conduction of heat is proportional to the rate of variation of temperature at point to point of the conductor; the other related to the distribution of electricity on conductors, and included the assumption that the electrical particles exerted mutual forces varying as the square of the distance. One implies a flow, the other instantaneous action. Yet these questions, so contrary in the experimental methods of investigation, were proved by Thomson to be mathematically one. Substitute in Fourier's formulæ electrical surface for heat surface, electric potential for temperature, and at once they are fitted to the use of the electrician.

And very much more than this collation of scientific principle, the highest of generalisation do we owe to the present occupier of the Professorial Chair at Glasgow. To Sir William Thomson are due the embodiment of Faraday's idea of continuous action in mathematical formulæ, the method of electrical images, and of electrical inversion. The practical applications of his mathematical theories are too well known to need detailing here, and the list is too long, for it includes researches into atmospheric electricity, the construction of electrometers, of galvanometers, telegraphic instruments, and experimental researches into all branches of electricity.

In Professor Thomson's book we really find the germ of much important work of our second author; for in one of Thomson's earliest papers—that on the attractions of conducting and non-conducting electrified bodies, published in 1843—we find the speculation commenced that Professor Maxwell has since made famous, the complete mathematical rendering of Faraday's physical lines of force. Then passing on to the consideration of vortex motion, we have Professor Maxwell again in the same field. But these developments are essentially original works, for so narrow and yet so liberal must be the reasoning that every step is an *opus magnus*. Here the resemblance between the two books ceases. Professor Maxwell's book, while it affords instruction to the student, really includes the most important of the higher theories of electrical science, the first issue being the treatment of Faraday's lines of force. How he has attempted this we must let our author speak for himself. The general complexion, he says, of the treatise differs considerably from that of several excellent electrical works, published, most of them, in Germany; and it may appear that scant justice is done to the speculations of several eminent electricians and mathematicians. One reason of this is, that before I began the study of electricity, I resolved to read no mathematics on the subject till I had first read through Faraday's "Experimental Researches on Electricity." I was aware that there was supposed to be a difference between Faraday's way of conceiving phenomena and that of the mathematicians, so that neither he nor they were satisfied with each other's language. I had also the conviction that this discrepancy did not arise from either party being wrong. I was first convinced of this by Sir William Thomson, to whose advice and assistance, as well as to his published papers, I owe most of what I have learned on the subject.

As I proceeded with the study of Faraday, I perceived that his method of conceiving the phenomena was also a mathematical one, though not exhibited in the conventional form of mathematical symbols. I also found that these methods were capable of being expressed in the ordinary mathematical forms, and thus compared with those of the professed mathematicians.

For instance, Faraday, in his mind's eye, saw lines of force traversing all space where the mathematicians saw centres of force attracting at a distance. Faraday saw a medium where they saw nothing but distance. Faraday sought the seat of the phenomena in real actions going on in the medium; they were satisfied that they had found it in a power of action at a distance impressed on the electric fluids."

The portions of this work relating to the construction of galvanometers and other electrical instruments show the author to be as much at home in the workshop as the scientific world knows him to be in his study.

The Year-Book of Facts in Science and Art. By JOHN TIMBS.
London: Lockwood and Co. 1873.

YEAR after year Mr. Timbs continues with praiseworthy industry to collect all kinds of scientific scraps, and to piece them together in the shape of these handy little volumes. The year-book for 1872, like its predecessors, contains an interesting collection of extracts which are generally well chosen; and though one may have met with most of the paragraphs elsewhere, it is convenient to have them at hand in a form easily available for reference. It is, however, to be wished that the compiler would extend his labours to the original sources of information offered by the Proceedings of our learned Societies; for the reader who consults his annals often craves for some higher authority than the daily and weekly journals which form the great repository whence Mr. Timbs draws most of his information. Still we have to thank the editor for taking the trouble to preserve in these annual records many useful paragraphs which would otherwise be soon lost in our ephemeral literature.

Mr. Timbs gives us, as usual, an obituary list of persons eminent in science, art, and literature, who have died during the year. A popular account is then given of most of the inventions in the mechanical and useful arts, and the principal discoveries in physical and natural science. The doings of the British Association at the Brighton Meeting come in for a full share of notice; and, finally, we have a table devoted to a summary of meteorological observations in 1872.

If Mr. Timbs's year-books cannot pretend to fully chronicle the progress of science, or to equal in completeness some of the German Jahrbücher, it must nevertheless be admitted that they are extremely handy books of reference, and may be fairly and favourably compared with the little yellow-covered volumes which we are in the habit of receiving from Paris—Figuier's "L'Année Scientifique et Industrielle."

Report on the Filtration of River Waters, for the Supply of Cities, as practised in Europe, made to the Board of Water Commissioners of the City of St. Louis. By JAMES P. KIRKWOOD, C.E. New York: D. Van Nostrand. London: Trübner and Co. 1869.

THE supply of wholesome water for domestic purposes is one of the most important questions of the present day, and in collecting and publishing the most recent experience as carried out in different towns of Europe, the Board of Water Commissioners of St. Louis have done a great public service; and from a perusal of the volume now before us we have no hesitation in stating that in the selection of Mr. J. P. Kirkwood for that duty their confidence has been in no way misplaced. The principal portion of this Report is devoted to the different forms of filtering beds used at the water-works in the principal towns of England, France, Germany, and Italy, whilst in the Appendices descriptions are given of the duty performed by the London pumping engines and their boilers, as an important subject in connection with the general question. The book is also copiously illustrated with well-executed engravings, which adds considerably to its value as a standard work of reference.

The amount of silt carried by rivers from which a water supply is drawn is, of course, an important question, as well as the nature of the districts through which they run, and the amount of contamination their waters receive from surface drainage off highly-manured fields and from the drainage of manufacturing districts. In a few places what is called the natural filter is in successful use. Where artificial filters are required, as is most generally the case, the materials used for their construction are sand, gravel, and broken stone or shingle, the depth of the whole varying from 5 to 6½ feet; a layer of cells has sometimes been used placed between the stratum of gravel, but this is not found essential, and is now generally omitted. In some cases this filter bed rests upon a foundation of puddled clay or concrete, where there is a loose and porous sub-stratum beneath. Each filter bed, at short intervals varying with the condition of the water, must have the deposit which accumulates on the surface of the sand cleared off or removed, and while any one is undergoing this process, the other remaining filters must be competent to deliver the required supply without overstraining their functions. If, then, there are six filters, five of them must be competent to the full duties of the service, and if eight filters, seven of them must be competent to this duty, on the supposition always that not more than one filter will at any time be off duty. Should the circumstances in effect render two unserviceable, the remainder must have area enough to meet the requirements of the case.

Space will not admit of our entering further into detail relative to the peculiarities of filter beds in different localities and under varying circumstances, but these will be found carefully considered in the pages of the Report itself, to which we must refer our readers if they desire more reliable information upon the subject.

PROGRESS IN SCIENCE.

MINING.

Gold Mining in the Colony of Victoria continues in a healthy state of activity, judging, at least, from the volume of "Mineral Statistics for 1872," which has been prepared as usual by Mr. Brough Smyth, and recently issued under the authority of the Minister of Mines at Melbourne. These statistics do not pretend to offer anything more than approximate figures, for the returns are given voluntarily, and there is therefore no means of obtaining them from those miners who are not sufficiently acute to see that the collection of such statistics must even be of the greatest benefit to the mining community at large, and as individual statements are merged in totals that can never be prejudicial to the interests of individual miners. But, though the Victoria returns may not state precisely the quantity of gold raised during the year, they evidently merit a considerable measure of confidence. According to the estimates made by the several mining registrars, there was obtained in 1872 about 1,331,377 ozs. of gold. The returns furnished by the Commissioner of Trade and Customs give 1,160,554 ozs. 19 dwts. as the quantity of gold exported during the year: whilst the Melbourne Branch of the Royal Mint has received 121.965 ozs. 17 dwts. Again, the returns made by the managers of the several banks showed that they purchased during the year 1,218,094 ozs. 9 dwts.

In studying the detailed statistics and comparing them with those of the previous year, it is satisfactory to note an increase in the average yield of gold from quartz, and also an increase in the quantity of stuff treated; there is, however, a slight falling off in the quartz tailings, mullock, &c., worked in the past year. Mr. B. Smyth calls attention to a cheap and simple, though slow, process for separating gold from pyrites, tailings, and mullock. By stacking such materials with a due proportion of small coal, gum leaves, and other vegetable matter, the gold would be slowly set free, and could then be readily collected. Appended to the volume of statistics is a report by Mr. J. Cosmo Newberry on the work done during the past year in the Melbourne laboratory.

The magnitude and value of the mineral resources of our Australian Colonies are well represented in the Australian Annexe to this year's International Exhibition. South Australia is especially prominent, exhibiting some splendid examples of copper and iron ores; the former including fine samples of native copper, red oxide, copper pyrites, purple ore, malachite, and atacamite from the mines on Yorke's Peninsula; whilst among the iron ores are some noble specimens of magnetite, hæmatite, and limonite. The South Australian copper industry is also represented by a metallurgical series from the Wallaroo smelting works. Bismuth ore from the Balhannah mine, and ingots of metallic bismuth smelted from this ore, are also exhibited; whilst the gold-fields, of which one has not yet heard much, are represented by some valuable specimens. The interest of this collection is greatly enhanced by the publication of an excellent catalogue.

New South Wales, in addition to its fine samples of coal, exhibits some splendid specimens of tin ore, illustrating the recent discoveries which were duly chronicled in these columns. Victoria sends some capital collections arranged by Mr. Brough Smyth, and well exhibiting the capabilities of the Colony. In the Queensland Annexe, an excellently arranged department, we note especially the fine specimens of precious opal recently discovered. This opal appears to occur in the form of a thin layer in fissures of ironstone nodules, but though of extremely fine colour with no lack of fire, it seems too thin to be of much value to the jeweller. We have lately seen some samples of opal, discovered under different conditions in the adjacent colony of New South Wales, but these are not yet represented in the Exhibition.

Some interesting information on the mineral resources of Upper Burmah is given by Captain G. A. Strover in a recently-issued official report. It appears that iron ore abounds in the Shan States, and that a manufactory on very crude principles is at work at Pohpah Toung. A rich hæmatite has been found abundantly to the west of Sagaing, and the requisite plant for establishing large works for smelting this ore is to be sent over from this country. Coal is known to occur in several localities in Burmah; some being, it is true, of only inferior quality, resembling lignite, whilst other varieties are said to equal the best English coal.

Several interesting papers bearing on Cornish mining were communicated to the Institution of Mechanical Engineers at its recent meeting in Penzance. Among them we may refer to a paper by Mr. J. H. Collins, "On the Mining District of Cornwall and West Devon," in which the writer gave a general description of the geological features of the district, the mode of occurrence of its mineral veins or lodes, and the methods of Cornish mining. In a paper "On Machinery for Dressing Tin and Copper Ores," Mr. H. T. Fergusson described in detail the different forms of tin stamps, and explained the advantages of Husband's patent pneumatic stamps; the author also noticed the chief improvements in the dressing of copper ores, including Borlase's buddle, Dingley's pulveriser, and Oxland and Hocking's patent calciner. A description of the tin steam works in Restronguet Creek, near Truro, was presented by Mr. C. D. Taylor, of Devoran. These works yielded large quantities of tin at the end of the last century, and after various vicissitudes have again been opened up by Messrs. Taylor. The Institution visited these works among other places of interest in the county.

Coal-cutting machinery has undergone considerable improvements at the hands of Messrs. Simpson and Hurd. After introducing many modifications, they have succeeded in producing a very superior machine. A couple of these machines—the first two which had been completed by the makers, Messrs. Matther and Platt, of Manchester—were recently exhibited at Wigan to the members of the South Staffordshire and East Worcestershire Institution of Mining Engineers. Some lithographs of the machine have been published in the *Colliery Guardian* (Aug. 8).

It is reported that recent explorations in the Hundred of Wirral in Cheshire, between the Dee and the Mersey, have led to the opinion that several seams of good coal exist within this area. A bed of coal, 11 feet thick, is, indeed, said to have been discovered on the Trelawney Estate at Songhall Massie.

We also hear of the discovery of a large lode of hæmatitic iron ore in North Devon, and of a rich silver-lead lode at Poolvash in the Isle of Man.

METALLURGY.

For the first time in the history of the Iron and Steel Institute, it has held a Continental Congress. Liège, on the Meuse, surrounded by some of the great iron-producing districts of Belgium, was the scene of its late session. In delivering the presidential address, Mr. I. Lowthian Bell gracefully alluded to the amicable relations between Belgium and Great Britain, and referred to the great value of the Belgian coal-fields, the exceptional character of their coal seams, and the ingenuity of her mining engineers in developing these resources under adverse natural conditions. It appears that during the past year the output of the Belgian collieries was about 14 millions of tons, and that between 5 and 6 millions were exported. The exports also included nearly 800,000 tons of hæmatitic iron ore, though iron ores are at the same time largely imported into Belgium. The president reminded our neighbours that the development of their iron trade was in large measure due to one of our countrymen, John Cockerill, who erected the first coke blast-furnace in Belgium, and thus laid the foundation of the great iron works of Seraing. Mr. Bell had a good word in favour of the *Ecoles des Mines* of Liège, Mons, and Charleroi, and for the kindred schools in other parts of Belgium. Nor did the excellent technical journal which issues from Liège—*Cuyper's Revue Universelle des Mines*—pass without its due measure of praise. Finally, the president took occasion to compare the iron industry of Belgium, France, and Germany with that of our own country.

In a very readable paper communicated to the Institute, M. Julien Deby traced the rise and progress of the iron and steel industries of Belgium. Going back beyond the ken of history, he said that archæological discoveries of quite recent date, still unpublished, seemed to indicate that at the period of the Roman Invasion iron had already been made in Belgium though unknown to the inhabitants of the British Isles. M. Piot has found in Brabant and elsewhere vast heaps of cinder, associated with stone arrow-heads and fragments of coarse pottery, the relics of a non-historic iron-working folk. In connection with the recent progress of the Belgian iron industry it may be stated that the first Danks's rotary puddling furnace was erected a few months back at the works of the Société Anonymé of Sclessin. As to the Belgian steel trade, which is comparatively of recent date, it appears that both Bessemer's and Siemens's processes are largely used, and that in 1872 as much as 15,284 tons of steel were made in the province of Liège alone.

A paper "On the Economical Preparation of Iron for the Danks's Puddling Furnace" was read before the Institute by Mr. C. Wood, of Middlesbrough. One of the difficulties of Danks's system is the mode of melting the pig-iron; if, as is commonly the case, the pigs are broken in halves and thrown direct into the rotary furnace, a long time is occupied in melting them, and during this time the furnace cannot be rotated, as any motion would evidently tend to knock the heavy bars against the lining of the converter, and thus cause injury. To obviate this inconvenience, it has been proposed to melt the iron in a cupola before running it into the rotator. But as this is an expensive method, Mr. Wood suggests that the pig-iron should be granulated by aid of the simple machine which he has successfully used for granulating blast-furnace slag. The revolving furnace is charged with the granulated pig-iron, and set in motion at once.

It is well known that for some time past, Dr. C. W. Siemens, F.R.S., has been actively engaged in developing his processes for the direct conversion of iron ores into wrought-iron and steel. His results were laid before the Chemical Society last spring in a valuable lecture "On Smelting Iron and Steel," which has been published in the July number of the Society's journal. It is unnecessary to offer an abstract of this lecture, as the metallurgical reader can so readily refer to the original, where he will have the benefit of consulting the accompanying illustrations.

Messrs. Gerhard and Caddich, of the Brierley Foundry, Bradley, have lately been turning out blooms of finished iron made direct from the ore by a new process. Ground hæmatite is mixed with fluxing and reducing agents in the form of lime and pitch, and the mixture baked in a coke oven. A furnace is charged with this preparation, and it is said that in half an hour the iron is turned out ready for the helve or the squeezers.

As all questions relating to the economy of fuel are of first importance to the metallurgist, we readily call attention to an excellent paper by Mr. Emerson Bainbridge, "On Coppée's Patent Coke Ovens, and the extent to which their Waste Gases can be Utilised," a paper recently published, with numerous illustrations, in the "Transactions of the North of England Institute of Mining and Mechanical Engineers." The iron manufacturer is so large a consumer of coke that any improvement in its manufacture closely affects his interest. The ovens almost universally used in this country for the preparation of coke are of that form known as the Beehive. Coppée's system is considered by the writer to present great advantages over these English ovens; but though it has been in operation on the Continent for at least a dozen years there have been but few of them erected in this country. The advantages of the Coppée ovens are—first, the retention in the form of coke of the largest possible proportion of the carbon of the coal; secondly, the utilisation of the heat of the evolved gases, by the use of flues so arranged as to impart an intense heat to the inside of the oven, and thus facilitate the expulsion of the gases; and thirdly, the application of the heat retained by the gases as they leave the stack of the ovens to the production of steam. No water is introduced into the ovens, as is done in the old system, but the coke is watered after it has been withdrawn, and thus absorbs about 3 per cent of water.

MINERALOGY.

A mournful interest clings to a memoir in a recent number of "Poggendorff's Annalen," descriptive of Gustav Rose's researches on the action of heat on diamond and graphite. This memoir represents, indeed, some of the last work in the long and active life of the great mineralogist of Berlin—a life which extends over nearly seventy-five years, and was brought to a close on the 15th of last July. Although it is beyond our purpose to trace the history of Rose's scientific labours, we may yet point to the long list of papers in the Royal Society's Catalogue—a list numbering upwards of 120 memoirs—in proof of his extraordinary activity, his devotion to original research, and the success with which he cultivated mineralogical science, whether in its geometrical, its chemical, or its geological aspect.

Rose's last paper, which was communicated a few months ago to the Berlin Academy, is the outcome of some lengthened researches on the behaviour of diamonds at high temperatures. To some extent these observations confirm those of Schrötter and others who have worked in this direction. For example, Rose found that by placing crystals of diamond between carbon-points *in vacuo*, and subjecting them to the action of a Siemens's dynamo-electric apparatus, the diamond became red-hot, and eventually flew to pieces, and at the same time the surface acquired a black crust which had all the characters of graphite. Exposed to the temperature at which cast-iron melts, the diamond was found to undergo no change, but at the fusing-point of wrought-iron it became quite black and opaque, exhibiting a strong metallic lustre, and becoming, in fact, converted into a graphitic substance. But the most curious point in the behaviour of diamond is seen when the gem is heated in a muffle, with access of air. Under these circumstances the faces of the diamond exhibit regular triangular depressions, reminding one of the markings common on many of the South African diamonds. Some interesting examples of these symmetrically developed etchings are figured in the plates accompanying the memoir in "Poggendorff's Annalen."

As so much discussion has been rife with respect to the nature of the colouring-matter of the emerald,—one party referring it to an oxide of chromium and another to an organic source,—it is interesting to find that the subject has lately been taken up by Mr. C. Greville Williams, F.R.S., who has communicated his researches to the Royal Society. On exposing a South-American emerald to a bright reddish-yellow heat for three hours, in a platinum crucible, the green colour was *not* destroyed; hence the author was led to disconnect the question of colour from that of the presence of carbon. Indeed a colourless Irish beryl was found to contain rather more carbon than a richly-tinted emerald. The author believes that there is no room for doubting the correctness of Vauquelin's conclusion, that the green colour of the emerald is due to the presence of chromic oxide. Experiments on the fusion of beryl and emeralds showed that these gems lose density when fused, but this fact cannot be used in argument against the formation of such minerals at a low temperature; for it is quite possible that they were crystallised from a fused mass which was originally formed at a temperature sufficiently high to keep the constituents of the emerald in a state of fusion, and that the crystals developed during a slow process of cooling.

Von Kobell has lately described, under the name of *Kjerulfin*,—a name suggested in honour of the Norwegian mineralogist and geologist, Kjerulf,—a new mineral-species from Bamle, in Norway. The substance had been determined by Rode, of Porsgrund, to be a new phosphate of magnesia, and complete analyses by Von Kobell and Wittstein lead to the following formula:—$2(3MgO.P_2O_5)+CaF_2$. Kjerulfin is therefore very similar in composition to the rare mineral known as Wagnerite.

Having had his attention directed to Wagnerite, by the study of Kjerulfin, Von Kobell has undertaken a new analysis of the old phosphate. From this recent examination of Wagnerite its formula seems to be thus represented :— $2(3MgO.P_2O_5)+(\frac{2}{3}Na,\frac{1}{3}Ca)F_4$.

A new mineral, belonging to the Pinite group, has been described by Laspeyres, of Aix-la-Chapelle, under the name of *Hygrophyllite*. This name refers to the curious behaviour of the mineral when brought in contact with water or with steam. Placed in water, the mineral—which is ordinarily of a greenish tint—immediately becomes white, and exfoliates in very fine scales, which peel off, layer after layer, until the entire mineral is disintegrated, and a whitish-grey finely-divided plastic mass is obtained, which, under the microscope, is seen to be made up of very delicate scales. In many other liquids, such as alcohol, ether, hydrochloric and nitric acids, the mineral retains its coherence.

Some "Mineralogische Mittheilungen" have been communicated by Dr. Wibel, of Hamburg, to Leonhard and Geinitz's "Jahrbuch." One of these communications describes the occurrence of the rare mineral, lime-uranite (autunite), in the so-called Portuguese phosphorite. The presence of a mineral containing uranium in a substance whose origin is so often referred to organic agency is not without its interest. The author has analysed a sample of gold from Vancouver Island, with the following results :—Gold, 91·86; silver, 6·63; copper, 1·00; iron, 0·51. A third subject discussed by Wibel is the composition and formation of blue carbonate of copper (azurite). His analysis of a Siberian specimen gave—Cupric oxide, 69·66; carbonic anhydride, 24·26; water, 6·08.

It is proposed by Von Kobell to change the name of the mineral called *Montebrasite* by Des Cloiseaux into that of *Hebronite*, the mineral having long been known from Hebron, in Maine, U.S.

The rare mineral called *Jordanite*, from the Binnenthal, has been lately studied by Sipocz, whose analysis points to the formula $As_2Pb_4S_7$. The same chemist also publishes analyses of a Hungarian Bustamite and an East-Indian potash-mica.

So much discrepancy may be observed in the analyses of the oxychloride of copper called *Atacamite* that we are glad to see that the mineral has been lately re-examined by E. Ludwig. His analyses of some fine crystals from Wallaroo, in South Australia, give results indicated by the formula $Cu_2ClO_3H_3$. The author believes the hydrogen to be essential to the constitution of the molecule of Atacamite, and that it exists in the form of hydroxyl. He constructs the following constitutional formula, making the atom of copper tetratomic :—

$$\begin{array}{c} Cu(OH)Cl \\ \| \\ Cu(OH)_2. \end{array}$$

The author's analysis of Brochantite also points to copper as a tetrad.

Atacamite has likewise been lately studied by Tschermak, who has specially directed his attention to the alteration by which this mineral can be transformed into malachite, some fine pseudomorphs illustrating this change having been obtained from the Siberian copper-mines.

Artificial crystals of Atacamite have recently been obtained by Friedel, whose process renders it probable that some natural forms of this oxychloride may have been produced by the action of ferric chloride on cupric or cuprous oxides. These results were obtained during the author's examination of a new mineral, *Delafossite*, which is a combination of the oxide of iron and copper, corresponding to $Fe_2O_3.Cu_2O$, or perhaps to the simpler expression $FeO.CuO$.

In some mineralogical notes on the Far West, Prof. Silliman puts on record the existence of *Enargite* in Southern Utah; and of *bismuthine*, *wulfenite*, *orpiment*, and *realgar*, from the same territory. He also describes a new borate of lime from Nevada, under the name of *Priceite*.

A chemical examination of Staurolite has been undertaken by Rammelsberg. Two crystals were examined—one from Brittany, and the other from Pitkäranta, in Finland.

The occurrence of indium in zinc-blende, from several American localities, has been detected by Mr. H. B. Cornwall. From Roxbury, in Connecticut, a blende was obtained so rich in indium that it could be detected spectroscopically by examining the raw powdered blende, without treatment with acids according to Richter's method.

Several analyses of American minerals have been published by Prof. A. R. Leeds, in a recent number of "Silliman's Journal."

A new analysis of *Dewalquite*, from Salm-Château, in Belgium, has been recorded by M. Pisani. This is the same mineral which has been described by German writers as *Ardennite*. The new analysis shows 3·12 per cent of vanadic acid.

ENGINEERING—CIVIL AND MECHANICAL.

Guns and Armour.—It is impossible to anticipate when or where the contest between guns and armour will cease. No sooner has an armour-clad vessel or fort been constructed, with a view to defence against the heaviest known guns, than the War Department at once begins to devise a gun whose shot shall pierce the thickest armour in the world. Thus there is continually going on an incessant competition for supremacy between the Admiralty and the War Office. The biggest gun of which we have hitherto heard is the "Woolwich Infant," of 35 tons weight, which fires a 700-lb. shot with a charge of 110 lbs. of powder; and the shot from this monster piece of ordnance could perforate the turret of the *Devastation* at any distance up to 500 yards. The armour plating of the *Devastation* is 14 inches in thickness, but an additional 2 inches in thickness would, it is said, render the vessel shot-proof against the biggest gun in the world. The War Department has hitherto cautiously advanced from guns of 12 tons weight, to 18, 25, and ultimately to 35 tons in weight; but they are now said to be contemplating the construction of one of 60 tons, the powder charge for which will weigh 200 lbs., and it will throw a shot over half a ton in weight, which will be able to perforate a 20-inch turret.

Harbours (*Holyhead*).—The inauguration of the harbour of refuge at Holyhead, on the 19th of August last, by the Prince of Wales, marks the completion of one of the finest works of this class yet completed. Between the years 1835 and 1847 the attention of Government was directed to the importance of providing improved harbour accommodation on the coast of North Wales, in the interest of the packet-service between England and Ireland. For this purpose Holyhead was selected as the most suitable site; and of the several schemes proposed for that place to accomplish the required end, the plan suggested by the late Mr. James Meadows Rendel was ultimately accepted, who, in August, 1845, was requested by the Lords of the Treasury to furnish detailed plans and estimates for the new harbour, and who reported thereon on the 5th of December of that year. Mr. Rendel's plan consisted of a north breakwater 5360 feet in length from the coast line, and an eastern breakwater about 2000 feet in length,—the two enclosing between them an area of 267 acres of available water space,—and of a packet pier 1500 feet long, situated within the enclosed area. The east breakwater was subsequently abandoned, as was also the proposed packet-pier, it having been subsequently determined to carry on the packet-service in the old harbour, where jetties and works have been constructed for the purpose. The north breakwater alone has been retained, and the new harbour has become principally a harbour of refuge. As the works advanced, however, it was found that, notwithstanding the abandonment of the new harbour as a packet station, which increased its capacity as a harbour of refuge, it was likely to prove too small even for refuge purposes. The Lords of the Admiralty therefore decided to lengthen the north breakwater by 2000 feet, and subsequently by another

500 feet, making its total length 7860 feet from the shore. These extensions considerably more than doubled the capacity of the harbour for refuge purposes, for they sheltered a roadstead of 400 acres of deep water, in addition to the 267 acres of water space. The breakwater consists of a sub-structure, or rubble mound, of stone, upon which is erected a substantial stone superstructure, the end of the breakwater being terminated by a head, on which is erected a lighthouse. The rubble mound is of great size, the average depth of water at low-water spring tides being 40 feet, and the greatest depth 55 feet, the rise of tide being 18 feet. The inclination given to the foreshore, or the slope from low water to the superstructure, is nowhere steeper than 7 to 1, and this inclination continues to about 10 feet below low-water mark, when the mound assumes a slope of 2 to 1, to about 25 feet below low-water mark, and somewhat flatter than 1 to 1 from that point to the bottom. On the harbour side the slope of the mound is about 1 to 1. At the level of low water the mound is nowhere less in width than 250 feet, and in 50 feet depth of water it is 400 feet wide at the base. It contains altogether about seven millions of tons of stone. The superstructure consists of a solid central wall of massive masonry, built principally of stone from the Holyhead mountain quarries. The foundations of this wall are laid at the level of low water, and it is carried up to a height of 38 feet 9 inches above low water, upon which is a handsome promenade, surmounted on the sea side by a massive parapet. At a lower level, or at 27 feet above low water, there is on the harbour side of the central wall a lower terrace or quay, 40 feet wide, formed by an inner wall built at a distance from the central wall, the intermediate space being filled in with suitable material. The head at the end of the breakwater is a massive structure, 150 feet long and 50 feet wide. The first contract for these works was made on the 24th of December, 1847. The late Mr. Rendel was Engineer-in-Chief from the commencement of the work until his death, at the end of 1856, when Mr. John Hawkshaw was appointed to that post, and the works have since been carried on under his superintendence. The cost of the whole of the works has been £1,479,538, which includes not only the outlay on the north breakwater, but also the provision of the accommodation for the Irish Postal Service in the old harbour, the construction of a beaching ground, and other miscellaneous works.

Kurrachee Harbour.—These important harbour works in India having recently been brought to a completion, a brief notice of them will be interesting. Kurrachee is situated near the north-western extremity of Sindh, and is the only seaport of that province available for vessels drawing more than 10 feet of water. Its position is one of very great importance, whether regarded from a commercial, political, or military point of view. In 1848 a lighthouse was erected on Manora Point, on the western entrance to the harbour; and, in 1855, in consequence of the increasing importance of the place, two dredging vessels were constructed for the improvement of the harbour, and a light-draught steamer was provided for the purpose of towing vessels in and out. In 1856 a committee was formed on the spot for the purpose of considering the best means of effectually improving the harbour, and their report was referred for the opinion of the late Mr. James Walker, C.E., who, in October, 1858, submitted his report, in which he recommended the construction of a breakwater from Manora Point, on the west side of the entrance; of a groyne from Keamari, at the eastern side; the closing of Chinna Creek, so as to force the ebb and flow tides to pass up and down the entrance channel, and the construction of new docks and basins, and of a graving dock. In 1860 orders were issued for the commencement of the works, which, with certain interruptions, have been carried on up to the present time,—first under the direction of the late Mr. Walker, and at his death by Mr. W. Parkes. Keamari Groyne is 7400 feet long, constructed of stone upon the Keamari sand-spit, having its top 2 feet 6 inches above monsoon high-water. This work was commenced in December, 1861, and completed in April, 1863. The extension of the groyne for 1500 feet (known as the East Pier) was commenced in May, 1864, and completed in October, 1865. Other works within the channel were also carried out which had the effect of considerably improving the harbour, but it was not until the year 1868 that the construction of the Manora Breakwater was sanctioned. This breakwater is 1500 feet in length, running out into the sea in a direct line from Manora Point. It is constructed of huge blocks of concrete resting upon a rubble base. These blocks are all formed of a uniform size and shape, and are held in position merely by their own weight, no cement of any kind being employed to join them to one another. The last block was set, and the Kurrachee Harbour Works thus far completed, on the 22nd of February last. By the aid of dredging over the bar, which runs across the mouth of the harbour, the result of these works is an increase of the depth of channel to 20 feet, and a considerably increased water-space within the harbour available for vessels to moor in.

International Communication.—M. Dupuy de Lôme has recently contributed to the Academy of Sciences a paper upon the scheme he has elaborated, in connection with Mr. J. Scott Russell, for improving the means of communication between England and France. According to the author of the paper, it is by the improvements in ships and ports that the desired end can be achieved rapidly, and without financial assistance from the State. The proposed solution to the problem involves the employment of very large vessels, suitable for carrying passenger and goods trains on board, as long since proposed by Mr. Fowler. The dimensions of these ships would be—Length, 442 feet; breadth, 35·75 feet; load draught, 11·48 feet; displacement, 2700 tons. They would be driven by side wheels 32·8 feet in diameter, actuated by engines of about 3600 horse-power. Each vessel would be able to carry a train, either for passengers or goods, 380 feet in length, and weighing 300 tons for the goods or 180 tons for the passenger train. The train would be run upon the after part of the ship, upon rails laid on the lower deck, and at a level of about 6 feet 6 inches above the sea. It would thus be covered by the upper deck, and be securely sheltered from the action of the sea. On either side of the line of rails suitable saloons and state-rooms would be provided for the accommodation of passengers. According to M. Dupuy de Lôme the steadiness of the boats would be greater than it is possible to obtain with ordinary ocean steamers: the mean intervals of the channel waves being from 7 to 8 seconds, the vessel should have a period of oscillation of from 12 to 13 seconds, so that one wave would counteract the rolling produced by the previous one. On the English coast Dover Harbour has been selected as a terminus; but as on the French side there exists no harbour like that of Dover, suitable for the entry of large vessels at all stages of the tide, it is proposed to create a port appropriate to the service of the train ferries, and the site selected by M. Dupuy de Lôme and Mr. Scott Russell is in the locality of Calais, the port being so laid out as not to produce any silting up around the entrances.

Dover Harbour.—The importance of improving Dover Harbour has at last become so urgent, both in consequence of requirements for improved accommodation for the Continental service, and of the necessities of the port for naval and military purposes, that the Government recently requested Mr. Hawkshaw to place himself in communication with Colonel Sir Andrew Clarke, with the view of considering whether a plan could be prepared which would combine the naval and military requirements of Dover with the objects desired to be effected by the Dover Harbour Board, and what the works would cost. The result has been a joint report from those gentlemen which, for the packet service, proposes the construction of a steam-packet pier about 1250 feet long by 125 feet wide, starting from a point close to the Admiralty Pier, and running in a south-easterly direction. Besides this a breakwater is proposed, which commences about 400 feet to the east of the Castle Jetty, and is continued seawards in a slightly south-westerly direction for about 3800 feet. At that point it turns and runs west for about 2200 feet, when it ends. Then comes an opening of 600 feet for vessels to enter, at the other side of which will be the end of the proposed extension of the present Admiralty pier, which is a length of 500 feet, and meets the end of the work now in progress at the present head of the pier. An opening of 900 feet is left in the eastern end of the breakwater. The estimate for the breakwater alone is, according to this design, £850,000, and the maximum time of construction eight years. Colonel Clarke has, however, proposed an alternative design, in which the railway company's water station is retained in a modified form. The proposed extension of the Admiralty Pier eastwards for 500 feet is retained, and then comes an opening of 600 feet. The breakwater then commences, and turns eastwards for about 2000 feet, when it turns by a curve, and takes a northerly direction to the shore, which it joins about 150 feet eastward of the Castle Jetty. The eastern entrance is omitted, as owing to the currents at that point it would in all probability silt up. The area enclosed is about 350 acres to low-water line. In the space left between the Castle Jetty and the eastern wall of the breakwater, Colonel Clarke proposes to build a small dockyard. From the Castle Jetty, along the whole coast line to the entrance of the present harbour, Colonel Clarke further proposes in the future to have a commercial quay and boulevard with trees and promenades. The whole matter is in the hands of the Board of Trade, and a sum of £10,000 was voted last session for carrying out the work according to Colonel Clarke's alternative design. The breakwater will be formed of concrete blocks, with the intervening space filled in with fluid concrete to a level of about 16 feet below low water. From that point and up to 3 feet above low water, concrete blocks alone will be used. From the last-named level to the top of the work, which is 6 feet above high water, the structure will be carried out in concrete, which will be put in between tides. The modification proposed by Colonel Clarke will admit of the completion of the work in five years from the time of their actual commencement.

Bow and Stern Screw Ships.—A paper on this subject was read on the 16th of June last, before the Royal United Service Institution, by Mr. Robert Griffiths, whose name is so well known in connection with the subject of screw propulsion. In order to avoid some of the difficulties and dangers that now attend screw ships, and also to improve their speed, it occurred to Mr. Griffiths that if in addition to the screw at the stern another propeller were applied in the bow of a steamship, both screws being placed in tunnels formed in the side of the ship so as to be protected from coming in contact with such objects as a ship's anchor or cable, it would be the means of avoiding a great many of the difficulties and dangers now attendant upon screw ships. From experiment it was ascertained that the best arrangement was that wherein the water from the bow screw was delivered underneath the ship, and water for the stern screw was taken also from underneath, so that both ends of the ship were made the same below the water-line. In this case the bow screw itself gave a better result in consequence of the water discharged from the screw meeting with a greater resistance, giving the same effect as is now produced by lowering the screw, and thereby obtaining a deeper immersion of the blades. Another great advantage may be obtained by this method of placing the screw, and that is, that the screw may be made to discharge any bilge water or any great leakage that may happen to take place in the vessel. As a result of trials made with a small vessel thus fitted with bow and stern screws, Mr. Griffiths asserts that besides numerous other advantages from this method of disposing the screws, 20 per cent less power is required by the screw when working in the tunnel to obtain the same speed than when working in the ordinary way.

Rock Drills.—A paper on the Diamond Rock Drill was recently read before the Iron and Steel Institute at Liège by Major Beaumont. The diamond drill is in principle quite distinct from any other system of holing rock, and works by rotation without striking a blow. Its action is rather that of abrading than cutting, and the effect is produced by the sheer difference in hardness between the diamond and the rock it is operating upon. The wonderfully resisting power of the diamond enables machinery of the simplest and most ordinary character to be used, and thus avoids those special difficulties that the mechanic must face when he is driven to utilise a large power in the production of percussive action; moreover, machinery can be applied in places where a reciprocating motion, if admissible at all, would present peculiar difficulties. The diamonds that are used are not valuable gems, but carbonate, a substance that till lately had no commercial value, and was first introduced for the purpose of cutting other diamonds. It differs from the brilliant diamond in being very imperfectly crystallised, which also gives to carbonate its value for drilling purposes, as it has next to no cleavage, and consequently does not split up or break in the way that a diamond or piece of boart would do. The application of the diamond to rock-drilling is worked out as follows:—The stones are set in an annular ring made of steel; they are fastened in by making holes as nearly as possible the size of the stones to be set, and then burying them, leaving projecting only the amount necessary to allow the water and *dêbris* of the cutting to pass; the metal is then drawn round the stone so as to close it in on every side, and give as large a bearing surface as possible to resist the tendency of the stone to be forced out. The crown so set is attached to the end of a steel tube and kept rotating against the rock at some 250 revolutions per minute. Water is supplied through the hollow of the bar, whence it passes under the cutting face of the crown to the surface of the hole between the side of the latter and the outside of the boring tubes; the diamonds are thereby kept cool, and the *dêbris* from the cutting is washed away. The crown has to be kept pressed forward with a force depending on the nature of the rock to be cut, varying from 400 lbs. to 800 lbs., when the cutting is done at speeds varying from 2 inches to 4 inches per minute. Granite and the hardest limestone are readily cut at 2 to 3 inches per minute; sandstone at 4 inches; and quartz at 1 inch per minute. The cutters travelling in an annular ring, it follows that a core is produced, an arrangement which, while it ensures a minimum of work being done to make a given sized hole, affords evidence of the strata passed through, a fact which is invaluable for certain applications.

GEOLOGY.

Palæontology.—Mr. Ray Lankester has described a new form of heterostracous fish-shield, which is intermediate in form between *Scaphaspis* and *Pteraspis*. The specimen, which he names *Holaspis sericeus*, is figured in the "Geological Magazine," and was discovered in the grey cornstone, of Old Red Sandstone age, near Abergavenny.

Professor Owen has communicated to the Geological Society the description of a fossil dentigerous bird, which he names *Odontopteryx toliapicus*. From a consideration of all the characters furnished by the remains, which were obtained from the London clay of Sheppey, he concluded that it was a warm-blooded feathered biped with wings; that it was web-footed and a fish eater, and that in the catching of its prey it was assisted by the pterosauroid armature of its jaws. He indicated the characters separating *Odontopteryx* from the cretaceous fossil skull lately described by Professor O. C. Marsh,* and which he affirms to have small similar teeth implanted in distinct sockets.

Professor Duncan, continuing his researches on the fossil corals of the West Indies, has now described those from the Eocene formation. He remarks that the affinities and identities of the fossil forms with those of contemporaneous reefs in Asia and Europe, and the limitation of the species of the existing Caribbean coral fauna, point out the correctness of the views put forth by S. P. Woodward, Carrick Moore, and himself concerning the upheaval of the Isthmus of Panama after the termination of the Miocene period.

In an address to the Natural History Society of Montreal, Dr. Dawson has discussed the geological distribution of the oldest known fossil, *Eozöon Canadense* and allied forms. He mentions its occurrence in rocks of Huronian age in Ontario and Bavaria; in the Middle and Upper Cambrian there are few limestones likely to contain such a fossil, but in Labrador species of *Archæocyathus* are found, one of which he has ascertained to be a calcareous chambered organism of the nature of a foraminifer, though there is little doubt that others are, as Mr. Billings has shown, allied to sponges. In the limestones of the Trenton group (Lower Silurian) animals of the Eozöon type occur abundantly. The concentrically laminated fossils which sometimes form large masses in these limestones, and which are known as *Stromatopora*, are mostly of this nature, although fossils of the nature of corals have been included with them. In the Upper Silurian (or Silurian proper) are similar if not identical forms known as *Cœnostroma*, with a skeleton consisting of a series of calcareous layers connected with each other by pillars or wall-like processes; while in the Devonian masses of limestone, sometimes 12 feet thick are made up of these organisms, which have clearly foraminiferal affinities, and are intermediate between the Eozöon of the Laurentian and the *Parkeria* and *Loftusia* of the greensand and eocene.

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* *Vide* Quart. Journ. Science, No. xxxviii., April, 1873, p. 272.

Prof. Bianconi has published further information on the bones of *Æpyornis*, corroborative of his views of its being an immense vulturine bird, the "Roc" of Marco Polo.

M. Schmidt, in a note published in the "Geological Magazine," states his opinion that the shields of *Pteraspis* and *Scaphaspis* belong to one and the same animal, *Scaphaspis* representing the ventral shield of *Pteraspis*.

Mr. Davidson has described some Brachiopoda collected by Mr. Judd from the Jurassic deposits of the east coast of Scotland. Three of them were obtained from the equivalent of the Kimmeridge Clay, which was the more remarkable as the Brachiopoda of that formation are comparatively few.

Dr. Gümbel has described to the Bavarian Academy of Sciences the so-called "Nullipores," which he resolves into two kinds:—1. True calciferous Algæ (*Lithothamnium*, and an allied form, *Lithiotis*); and 2. Foraminifera (*Dactyloporideæ*).

M. Barrande, who is so well known for his researches on the Silurian system of Bohemia, has recently published a supplement to Vol. I. of his great work, on the different Crustacea and Fishes of these old rocks. Of the former he describes ninety-four new species of Trilobites, of the latter indications of four genera have been discovered, namely *Asterolĕpis*, *Coccosteus*, *Ctenacanthus*, and *Gompholepis*. His observations do not agree with the theory of evolution. He passes in review the different parts of the Trilobites, the succession of their species and genera in time, and institutes a comparison between the fishes, Trilobites, and Cephalopoda, and their relations to the primordial fauna generally. Everywhere he finds that the appearance of new forms is sudden and unaccountable, and that there is no indication of a regular progression by variation.

The base of the Palæozoic series in America until lately was formed by the Potsdam sandstone, although lower horizons of life had been determined by Barrande in Bohemia, and Salter and Hicks in Wales. The researches of Mr. Murray in Newfoundland, together with the study of the fossils by Mr. Billings, have revealed a lower Potsdam, while Messrs. Hartt and Matthew, by their explorations of the rich primordial fauna of St. John, have led to the establishment of an "Acadian Group" on the horizon of the lower slate group of Jukes in Newfoundland, of the gold-bearing rocks of Nova Scotia, and of the slates of Braintree in Massachusetts.

Stratigraphical Geology.—One of the most important papers in British geology which has been published in recent years is that "On the Secondary Rocks of Scotland," by Mr. J. W. Judd, the first part of which has been published in the "Quarterly Journal of the Geological Society." The Mesozoic periods are in Scotland represented only by a number of isolated patches of strata, situated in the Highlands and Western Isles; which have been preserved from the destructive effects of denudation, either through having been let down by great faults among the palæozoic rocks, or through being sealed up under vast masses of tertiary lavas. These have been unravelled by Mr. Judd, who has depicted their superficial extent upon a coloured geological map which accompanies his paper. The cretaceous rocks, exhibiting very interesting characters and yielding a beautiful series of fossils, were discovered by the author on the mainland, and in several of the islands of the west of Scotland. The Jurassic rocks, which were first described by Murchison, are now shown to present a remarkable contrast with their equivalents in England, in being constituted throughout their whole thickness by alternations of marine and estuarine series of beds; in which respect they precisely resemble the equivalent strata of Sweden. The Triassic rocks have now been discovered in Sutherland, where their conformable relations to overlying beds, containing a fine Liassic fauna, entirely confirms the conclusions concerning their age, derived from Professor Huxley's studies of the remarkable reptiles yielded by them in Elgin. Two new species of Brachiopoda, discovered by Mr. Judd in the Upper Oolite of Garty, in Sutherland, are named by Mr. Davidson respectively, *Rhynchonella Sutherlandi*, after the Duke of Sutherland; and *Terebratula Joassi*, after the Rev. J. M. Joass.

Physical Geology.—The Rev. J. M. Mello, in a sketch of the geology of Derbyshire, touches upon the solvent power of water on the mountain limestone as explaining the origin of its characteristic scenery. He considers that the dales were in many instances originally caverns, which have been through countless ages eaten away by the streams till at length the roofs have fallen in, and in their turn have been for the most part carried away by the same powerful agent.

The Rev. O. Fisher, in treating of the formation of mountains, has attributed the elevating force, which has raised mountain ranges, to the contraction of the heated interior of the earth, and subsequent wrinkling of the crust so as to accomodate itself to the diminished nucleus. In a recent paper, communicated to the "Geological Magazine," he proves that if we suppose a stratum 500 miles thick, buried under 25 miles of crust, to have contracted since the crust became rigid on the whole, as much as a slag would do in passing from a fused to a devitrified state, this would give a mountain-range of something under half a mile high on every hundred miles of surface. If only a part of the area were disturbed the mountains would be higher.

Mr. W. T. Blanford has drawn attention to the superficial deposits of Persia. He described especially the desert plains of the interior of the country, the paucity and scantiness of the streams, most of which terminate in salt swamps and lakes, and the occurrence of vast slopes of gravel on the margins of the desert plains, covering up the junction of the latter with the surrounding mountains. The desert plains he regarded as in general the beds of ancient lakes.

Glacial Geology.—The Duke of Argyll, in his Presidential Address to the Geological Society, while discussing the general opinion in regard to the glaciation of the British Isles, remarked "that the history of geology, like the history of other sciences, is the history of the prevalence of particular theories at particular times—not generally to be wholly abandoned, but almost always to be greatly modified." He had "a strong impression that the glacial theory is now at about its maximum, and that, when all our valley-systems are described as being nothing but magnified striæ, we are pretty near the summit-level of this particular excursion of the scientific imagination."

Dr. Dana's observations upon the Glacial and Champlain Eras in New England, go to show that the former was an era eminently of transportation by ice, the latter one of deposition. He regards the Glacial period as of great duration, and expresses the opinion that 1 foot a week was the average rate of the movement of the ice, so that 10,000 years would be required to carry a boulder 100 miles. In the northern part of New England, he estimates the ice to have had a thickness of from 5000 to 6500 feet, and in the southern part an average of 2700 feet. The pressure must have been immense—6000 feet corresponding to at least 300,000 pounds to the square foot; the glacier as it moved must have had tremendous power in abrading, and made boulders and gravel in immense quantities.

Mr. J. C. Ward has described the glaciation of the Northern part of the Lake district. He maintains that there is no evidence that a great ice-cap from the north ever swept over this district. The ice scratches trending along the principal valleys, but sometimes crossing watersheds, indicate a great confluent glacier-sheet, at one time almost covering a great part of the district, the movement of which was determined by the principal watershed of the Lake district.

Mr. J. F. Campbell has described the Glacial phenomena of the Hebrides. Various ice-marks were noticed, which all seemed to come from the north and west, also numerous perched blocks. On the whole, the author was inclined to think that the last Glacial period was marine, and that heavy ice came in from the ocean, the local conditions being like those of Labrador. He regarded most of the Lake-basins of the Hebrides as formed by ice-action, and considered that the ice by which those islands were glaciated came from Greenland. Mr. James Geikie has also described the Glacial phenomena of Long Island, or Outer Hebrides. The lakes of the mountain district he regarded as all produced by glacial erosion.

The Duke of Argyll has discussed the formation of six Lake-basins in Argyllshire, five of which he considers could not have been due to glacial action.

The glaciation of the British Isles has received of late a good deal of attention, and the presence of sheets or fields of ice covering vast areas has been invoked in order to account for the phenomena observable in different parts of the Kingdom. Mr. R. H. Tiddeman, describing the glacial phenomena of North Lancashire, and the adjacent parts of Yorkshire and Westmoreland, as bearing testimony to a widespread and almost universal glaciation in the district, brings forward evidence to show that they were produced by an ice-sheet. While, however, the drainage of the district is to the south-west, the general movement of the ice over it appears to have been to the south or south-south-east, across deep valleys, and over hills of considerable elevation. This he explains by the scratches on the rocks, the direction and method of transport of the Till, its materials, and their arrangement along lines coinciding with the scratches, as well as by the superficial disturbances of the rocks. To account for the direction of the ice-sheet, which under ordinary circumstances would be working down from the watershed to the sea in the direction of the main valleys, Mr. Tiddeman considers that there must have been a great barrier, along what is now the seaside plain, to dam up the mouths of these valleys to a great height, and prevent their discharge of ice to the south-west. Evidence of such a barrier exists in the traces of a great stream of ice coming from the Lake district and bearing with it rock specimens of that country. This barrier he concludes was but the line of junction of the ice of the Pennine chain with that from the Lake district, and to the eye they must have presented only the appearance of one great sea of ice. These observations, which Mr. Tiddeman communicated to the Geological Society of London, are illustrated by a coloured map upon which the physical features of the country are depicted and the direction of the ice-scratches shown.

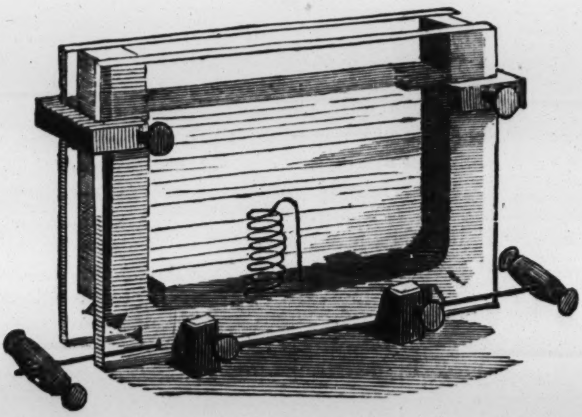
PHYSICS.

Microscopy.—The most powerful spectroscope yet constructed has been presented to the University of Oxford by Mr. J. P. Gassiot. The great dispersive power of the instrument is obtained by a battery of six compound prisms 3 inches high by 2 inches wide. The light, after passing through the upper half of these prisms, is reflected back through the lower half, the light in its course through the prisms having to pass through more than 4 feet of glass before it reaches the eye of the observer. The telescopes are of 18 inches focal length, and the object-glasses 1¾ inches in diameter. The prisms are provided with the automatic arrangement for keeping them at the minimum angle of deviation for any ray under examination. It is intended that all the measuring of the spectra should be done by means of a micrometer eye-piece placed in the telescope; but for the purpose of readily finding any line in the spectrum, the prisms are provided with a vernier which moves round a circular arc; the divisions are on an alloy of palladium with silver. There is a contrivance for setting the train of prisms in motion, the milled head which moves the prisms being close to the eye-piece of the telescope, and thus completely under the command of the observer. The weight of the instrument is rather more than 140 lbs.

Mr. J. W. Stephenson, F.R.A.S., and Mr. Charles Stewart, F.R.M.S., make use of the appearances presented by objects immersed in media of different refractive power to determine some points in their structure.* This especially applies to colourless transparent organisms such as the skeletons of diatoms and siliceous and calcareous spicules of sponges. The siliceous deposits, both of plants and animals, are of less refractive index than Canada balsam ; consequently, when mounted in that medium they appear, if convex, to act as concave lenses do in air, and *vice versa*. If diatoms are examined in air, *i.e.*, dry, they are in some instances too opaque for transmitted light, but on immersing them in water, of which the mean index is 1·366, they become more translucent ; with media of higher refractive power, the translucency increases until the mean index of strong sulphuric acid (1·434) is attained, in which they become practically invisible. As every object which is transparent and colourless becomes absolutely invisible when immersed in a colourless medium identical in refractive power with itself, we know approximately that the refractive index of diatomaceous silex is 1·434 (much below that of quartz), and this is accordingly for diatoms our neutral point. By progressively increasing the refractive power of the mounting medium the diatoms again become more and more visible, until, as we all know, when mounted in Canada balsam (1·540) the coarser species are sufficiently defined for all ordinary purposes ; but if we require a still greater departure from the neutral point, or invisible condition, we must select some other substance of still higher refractive power. This we find in bisulphide of carbon, the index of which is 1·678, and by dissolving phosphorus in the bisulphide we may obtain any power between 1·678 and 2·254 ; but when large thick diatoms, such as *Heliopelta*, are mounted in a strong solution of phosphorus, they again become nearly, if not quite, as opaque as they were in air. From the above it is evident that, on examination of a diatom or other object in air and in bisulphide of carbon, they are seen under conditions in which the respective optical effects arising from their form are reversed. The results of the examination of some diatoms are given in the paper, with figures showing the various effects produced by the use of varied mounting media. It is also suggested that some animal tissues, in which the staining process has failed to reveal differences of structure, may be profitably examined in media of such high refractive index as bisulphide of carbon or oil of cassia.

Lecture Illustrations of Solar Phenomena.—In a recent lecture on "Sunlight and its Source," President Morton, of the Stevens Institute of Technology, employed several new illustrations of his own device, which will interest some of our readers. In the first instance, to illustrate the motion of sun spots across the solar disc, their foreshortening when near the limb, &c., an apparatus was employed consisting of a glass cylinder, on which a sun spot

FIG. 4.



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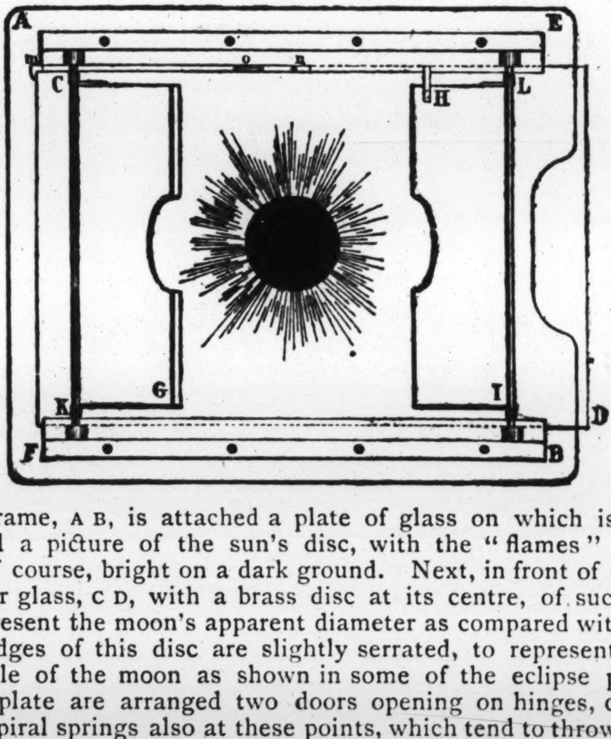
* Monthly Microscopical Journal, vol. x., p. 1.

was painted, supported so as to rotate on its axis and admit of various inclinations. This was placed as an "object" in a large oxyhydrogen lantern, in front of a circular orifice representing the solar disc. The effect upon the screen was remarkably good. To imitate the formation of solar protuberances, a glass tank, provided with a coil of platinum wire, was likewise introduced in the lantern and filled with water, at the bottom of which was a little solution of cochineal. The coil being heated by the current from a single "flask" battery, a stream of the crimson cochineal solution was thrown up, and assumed from time to time various forms, some of which bore a striking resemblance to the figures of solar prominences which have been figured by Young, Lockyer, Respighi, and others. Again, to illustrate the various phenomena of a solar eclipse, an apparatus was employed whose construction and operation can be explained by the aid of the accompanying figure. To the further

FIG. 5.



side of the frame, A B, is attached a plate of glass on which is painted or photographed a picture of the sun's disc, with the "flames" and corona. These are, of course, bright on a dark ground. Next, in front of this, slides a plate of clear glass, C D, with a brass disc at its centre, of such size as to correctly represent the moon's apparent diameter as compared with that of the sun. The edges of this disc are slightly serrated, to represent the mountainous profile of the moon as shown in some of the eclipse pictures. In front of this plate are arranged two doors opening on hinges, C K, and L I, and having spiral springs also at these points, which tend to throw them open. The door, H I, a little overlaps the other, and thus a bolt, *m o n*, engaging a projection at H, secures both doors when they are shut. A circular orifice, half in each door, corresponds with the solar disc on the rear glass. The doors then being shut, and the plate C D drawn to the right, we see only the bright solar disc with such sun spots and faculæ as have been represented on it. Then the plate C D being slowly pushed towards the left, we see the moon's disc encroaching on the sun, and all the phenomena of the partial phases, ending with "Baily's beads," which are of course due to the serrations of the disc. An instant after, and as the disc entirely shuts off the sun, the glass plate, C D, by pressing against the lug, *m*, of the bolt, shoots it, and allows the doors to fly open, displaying the prominences and corona surrounding the dark lunar disc. The figure represents the doors in the act of flying open, and is correct in detail, except that the corona should appear bright on a dark ground. Yet again, in illustration of the vivid brightness of the crimson protuberances or hydrogen flames, as seen during a total eclipse, the following device was employed:—A large coloured drawing of the moon, surrounded by the solar corona, was stretched on an appropriate frame, and the places of the prominences cut out. Behind these were attached a number of hydrogen spectrum tubes, and the whole enclosed from behind with a board covered with white paper. The reflection from the white paper fills the whole opening representing the prominence with crimson light, so that the tubes are not noticed. Fig. 6 shows the general appearance, and Fig. 7 the arrangement of the tubes. President Morton also stated that he was preparing some spectrum tubes with meteoric hydrogen, so as to have what might be considered as a real solar-prominence specimen.

FIG. 6.

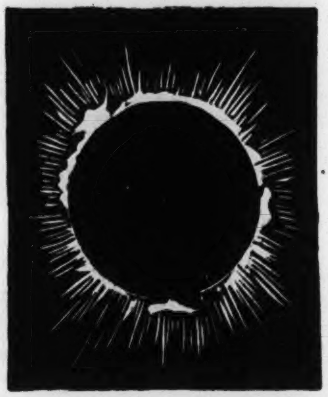
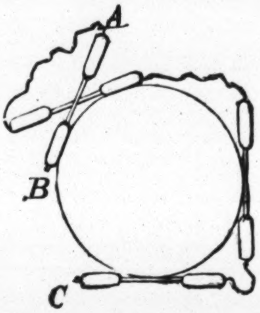


FIG. 7.



ELECTRICITY.—The improvements in magneto-electric machines follow each other in rapid succession. Hardly is M. Gramme's machine announced ere Mr. Wild, who has long been known in electrical circles as one devoting great attention to, and bringing forward important inventions in, magneto-electricity, introduces a machine with multiple armatures, producing a greater number of currents for one revolution of the axis than has hitherto been obtained. Although this machine has for many practical purposes been superseded by the continuous current generator of M. Gramme, its importance in the production of the electric light must not be overlooked. It consists of a circular framing of cast-iron firmly fixed by stay-rods. A heavy disc of cast-iron is mounted on a driving-shaft running in bearings fitted to each side of the framing; one of these bearings is carefully insulated from the framing by ebonite, and also from the shaft by a cylinder of the same substance. Through the side of the disc, and parallel with its axis, sixteen holes are bored for the reception of the same number of cores or armatures. Around each inside face of the circular framing, and concentric with the driving-shaft, sixteen cylindrical electro-magnets are fixed; the two circles of magnets consequently have their poles opposite each other, with the disc and its circle of iron cores revolving between them. The ends of the cores are terminated with iron plates of a circular form, which answer the double purpose of retaining the helices surrounding the cones in their places, and overlapping for a short distance the spaces between the poles of the electro-magnets. The closing of the *magnetic* circuits of the electro-magnets and armatures for a short distance, like the closing of the *electric* circuits for a brief interval at the point of no current, has a marked influence on the power of an electro-magnetic induction machine, both contrivances conspiring simultaneously to maintain the magnetic intensity of the electro-magnets during the rise and fall of the magneto-electric waves transmitted through the helices. With the Gramme machine much progress has been made in detail. One of the improvements, by Mr. Robert Sabine, C.E., tends to add mechanical, and somewhat of electrical, strength to the apparatus. Instead of the cumbrous magnets employed by the inventor, Mr. Sabine proposes to substitute magnets placed parallel to the coil (see Figs. 3 and 5 of our article on "Magnetic Illumination" in the July number of this Journal) and united at their similar poles by transverse bars of soft-iron. The magnetic polarity of these bars follows precisely the Faradian lines of force, and the true pole appears to be situated in, or rather shifted to, the point where the bar would most influence the coil. The advantages gained in economy of construction (by the use of smaller magnets) and in compactness of arrangement are the chief points in the improvement. With the Gramme machine a striking experiment, first made by MM. Gaston-Planté and A. N. Breguet, has been recorded by them in "Les Mondes." They charge a Planté secondary couple (coiled sheets of lead in dilute sulphuric acid), and, in place of discharging the couple, they allow it to remain in communication with the machine; when, if the machine be suddenly and completely stopped by the hand, the coil, on the removal of the hand's resistance, will re-continue to revolve, not in an opposite direction, but in the same direction as when charging the secondary couple, for a period of two or three minutes. Nothing would appear more paradoxical than that the machine should continue to turn in the *same* direction. But the explanation is simple, and is given by the experimenters in the following words:—"If we consider the direction of the current furnished by the machine that of the current given back by the secondary couple, which is inverse to the preceding, and if we take into account the action which results, we shall find that, according to the laws of induction and electro-dynamics, the movement of rotation should be in the direction indicated by experiment. And if we observe that the secondary couple has an intensity temporarily superior to that furnished in a given time by the machine, we shall easily be able to perceive that the discharge from the secondary couple overcomes that of the machine."

The next step in electrical science most probably will be in improved application of the electric light. Recently, at St. Petersburg, experiments have been made by Messrs. S. A. Kosloff and Co. with the invention of M. Ladiguin. This invention consists in the use of one piece of charcoal or other bad conductor, which, being attached to a wire connected with a magneto-electric machine, is placed in a glass tube from which the air is exhausted, and replaced by a gas which will not at a high temperature combine chemically with the charcoal. This tube is then hermetically sealed, and the machine being set in motion, the charcoal becomes gradually and evenly heated, and emits a soft, steady, and *continuous* light. Taking into consideration the fact that one machine worked by a small three-horse power engine is capable of lighting many hundred lanterns, it is evident that a great step has been gained to the end of lighting our streets by means of electricity.

Practically, perhaps, the greatest good has been achieved by the application of the duplex system of working on our telegraphic lines and cables. By this system the capacity of our lines for work are doubled, and consequently the capital invested should repay doubly what it would on the single transmission system. The improvements in this country come chiefly from Mr. W. H. Preece, C.E., of the Post Office telegraphs. The Americans have long employed a system similar in effect, invented by Mr. J. B. Stearns. The system apparently most in favour is that founded on the use of the Wheatstone bridge; and, indeed, the system may be described briefly as the arrangement at each station of such a bridge system as will have its galvanometer needle deflected by a current other than that of its own battery. This is effected by substituting the instrument of the sending station for the galvanometer of the bridge, putting equal or proportional resistances into two branches, the line wire into the third branch, and a resistance compensating that of the line wire into the fourth. An out-going current thus possesses opposite and equal potentials at the terminals of the instrument, but an in-coming current finds only a divided circuit, in one branch of which is the instrument.

Amongst the electrical apparatus meeting the wants of the laboratory and lecture-table especially, may be classed the new ozone-generator invented by Mr. Tisley. The construction of this instrument may be described as follows:—On each end of a piece of glass tube of uniform bore is placed a brass cap, bored with two holes, and coated internally with shellac. In the interior of this glass tube, and of a diameter scarcely less than that of the tube itself, but not quite so long, is placed a thin hollow brass box, with its surface made as true as possible by turning in a lathe. This brass box is placed concentrically with the outer tube, and is completely coated on its exterior surface with tin, the tin being acted on to the smallest extent by the ozone. This hollow box communicates with the exterior of the apparatus by means of tubes passing through the centre of the caps. It is intended that a current of water should circulate through this box. In the small annular space between the box and the glass tube oxygen is passed from the tubes fitted to the cap. The box itself forms one of the electrodes in connection with an induction coil, and a strip of tin-foil fixed to the outside of the glass tube forms the other.

Another interesting but different order of experiment, a discovery of M. Demoget, consists in replacing the plate of resin of an electrophorus by a membrane of caoutchouc. The membrane from a child's air-balloon strained on a metallic circle of 0·8 c.m. diameter, smartly rubbed with the back of the hand, the rubbed surface inverted upon a good conductor and the superior surface rubbed, will result in imparting to an insulated disc of 25 c.m. diameter sufficient electricity to discharge sparks of 3 to 5 c.m. length. The experiment may be utilised in the charging of an electrometer, or may be the source of amusement to the young.

The electrical theories advanced by M. Edlund, and that of electrolysis by M. Domalip, during the last quarter, would require more space for adequate consideration than can be afforded to a *resumé*.

M. Dupuy de Lôme describes the cryptograph of M. Pelegrin as an instrument designed to be raised on the ground, and to convert into expressions capable of being transmitted directly and secretly by telegraph, the polar coordinates of points which determine a given figure; whence the possibility with this instrument of following and interpreting, that is, of drawing, in Paris, *e.g.*, what correspondents in different parts of the country in telegraphic communication with Paris may see and telegraph but do not interpret.

TECHNOLOGY, &c.

In consequence of the illness of Dr. Joule, Dr. A. W. Williamson, F.R.S., presided over the Bradford Meeting of the British Association for the Advancement of Science. His Inaugural Address, which attracted the deepest attention, was delivered on the evening of the 19th of September. Owing to the meeting being held later in the season than usual we are prevented from giving an abstract of the proceedings in our present number. Evening lectures were delivered by Prof. W. C. Williamson, on "Coal and Coal-Plants;" by Dr. Siemens, F.R.S., on "Fuel;" and by Prof. Clerk Maxwell, F.R.S., on "Molecules." Dr. Russell, F.R.S., was President of the Chemical Section. He departed from the usual custom of reviewing the progress of chemistry during the year; and after a feeling allusion to the death of Liebig, and to his connection with the British Association, the attention of the audience was directed to the history of the colouring matter found in madder. Of the papers read before the Association, that which deservedly attracted the greatest attention was Dr. Ferrier's paper on the "Brain." We shall give an authentic account of these important researches in our next number.

The American Association for the Advancement of Science held its twenty-second annual meeting at Portland, Maine, beginning Wednesday, August 20th. The Officers of the meeting were—President, Prof. Joseph Lovering, of Cambridge, Mass.; Vice-President, A. H. Worthen; Permanent Secretary, F. W. Putnam; General Secretary, C. A. White. The attendance of members was large, and a goodly number of papers were presented. The following papers on chemical and closely related subjects were read:—"On the Silt Analysis of Soils and Clays," E. W. Hilgard; "Analysis of Mississippi Soils and Subsoils," E. W. Hilgard; "On the Distribution of Soil Ingredients in the Sediments obtained by Silt Analysis," R. H. Loughbridge; "On the Influence of Strength of Acid, and Time of Action on the Results of Chemical Soil Analysis," R. H. Loughbridge; "Remarks on Plate Lime-Glass and the Manufacture of Glass in General," L. Feuchtwanger; "The Chemistry of Copper Matte," T. Sterry Hunt; "Metaphysical Theory of Chemistry *versus* the Atomic," Clinton Roosevelt. Subjects connected with geology and natural history formed the bulk of the papers.

At one of the "Public Conferences" of the French Association for the Advancement of the Sciences, which was also held in August, at Lyons, M. A. Gaudry delivered a lecture on the modern progress of chemical industry. He informed his audience that the amount of sulphuric acid manufactured annually in Europe amounts to 800,000,000 kilos., and would fill a canal 2 metres deep, 10 wide, and 25 to 30 kilometres in length. To yield this acid 800,000 tons of pyrites are yearly consumed. The condensation of the hydrochloric acid liberated in alkali works, the improvements of Mr. Weldon and Mr. Deacon in the manufacture of chlorine, the revolving soda-furnace, the extraction of potash as a secondary product in the manufacture of beet-root sugar, and the recent improvements in producing paper-pulp from wood, are among the principal points touched on in the remainder of this popular and able lecture.

M. Ruimet des Taillis, writing in the "Chronique de la Société d'Acclimatation," states that, by feeding silkworms on vine leaves, he has obtained cocoons of a magnificent red, and, by employing lettuce, others of a deep emerald-green. M. Delidon de Saint Gilles, of Vendée, has obtained silk of a beautiful yellow, other samples of a fine green, and others again of a violet, by feeding the silkworms on lettuce or on white nettle. He points out that the silkworms must be fed on mulberry leaves when young, and supplied with the vine, lettuce, or nettle leaves during the last twenty days of the larva-stage of their life.

For the preservation of gum arabic from mouldiness Hirschberg adds a little sulphuric acid to the solution, and finds that the mixture retains its adhesive property uninjured after the lapse of eighteen months.

M. Ducrot has published an interesting paper on apparatus for heating with hot air. From the theoretical point of view, the writer concludes that the quantity of calories furnished by the same apparatus, acting under the same conditions, is greater as the heated air issues at a lower temperature. By the same conditions, he means a constant external temperature, same quantity of fuel disposed in the same manner on the grating, burned in the same time with equal quantities of air. There is, however, for a piece heated with a given weight of fuel per hour, a maximum of temperature corresponding to a determinate quantity of air passing over the heating apparatus.

The following process was proposed by the late Professor Fuchs for fastening leather upon metal:—One part of crushed nutgalls is digested six hours with eight parts distilled water, and strained. Glue is macerated in its own weight of water for twenty-four hours, and then dissolved. The warm infusion of galls is spread upon the leather, the glue solution upon the roughened surface of the warm metal, the moist leather is pressed upon it and then dried, when it adheres so that it cannot be removed without tearing.

Steam has been proposed for extinguishing fires, by means of large pipes, communicating with a boiler, and capable of filling the building with steam in case of a conflagration.

In a paper on the spontaneous combustion of hay, H. Ranke says that, in consequence of prolonged fermentation, hay can become transformed into a true coal, which, when exposed to the air at somewhat elevated temperatures, acts as a pyrophorus.

M. Jobert has instituted researches on the history of digestion in birds: he finds that the gizzard is not exclusively a triturating organ, but a chemical stomach, which secretes an acid liquid.

According to E. Brescius beer may be clarified by means of tannin. For 1000 litres the author employs about 140 grms. of tannin, dissolved in 0·75 litre of water, which is thoroughly stirred up. After three or four days he adds 1 litre of isinglass or 2 of gelatine in the proportion of 1 kilo. to 100 litres. The complete clarification requires about eight days.

Struck with the inconveniences resulting from the use of toilet-soaps with a base of potash or soda, M. Bonnamy has prepared alumina soaps. These are, as a matter of course, neutral and free from causticity, and being insoluble in water their detergent action is simply mechanical, not chemical.

Ozokerit, which is now largely used in the manufacture of candles, is found in beds in the sandstone of Slanik, in Moldavia, in the neighbourhood of mines of coal and of rock-salt; it has also been discovered in the Carpathians. The material in its crude state is brown, greenish, or yellow; it is translucent at the angles, and its fracture is resinous. It is naturally brittle, but when softened can be kneaded like wax. It blackens on exposure to the air. It becomes negatively electric on friction, and exhales then the aromatic odour of a hydrocarbon. It melts at the low temperature of 66°. Its illuminating power is such that 754 ozokerit candles give a light equal to 891 of paraffin, or 1150 of wax.

The recent sudden destruction of two large passenger ships, the *Atlantic* and the *City of Washington*, has called attention to the desirability of availing ourselves of the means which modern science has placed at our command for the prevention of such disastrous accidents. For this purpose Mr. John Newlands proposes that each large passenger ship should carry a small but powerful steamboat or launch, and in foggy weather this steam launch should be sent on ahead some few hundred yards, being connected with the passenger ship by a flexible telegraphic cable provided with an electric battery, so that signals or messages might be continually transmitted from one to the other. The steam-launch should also carry an electric or other strong light, and be provided with a powerful steam whistle. On meeting with ice or with vessels, or unexpectedly approaching the coast, it would be comparatively easy to stop the steam-launch and give warning in time to save the passenger ship from danger.

General Morin gives a formula indicating what amount of air should be renewed hourly for each individual, in order that carbonic acid and vapours exhaled may not accumulate beyond a proportion of 0·0008 in a given enclosed space. He finds that in a cubic space of 10 cubic metres this renewal hourly should be 90 cubic metres; in 12, 88; in 16, 84; in 20, 80; in 30, 70; in 40, 60; in 50, 50; in 60, 40. Various applications of the formula are suggested—barracks, bedrooms, public halls, hospitals, &c.

Some improvements in photo-lithography have been effected by M. Paul. The paper is coated with a layer of white of egg beaten up and mixed with a concentrated solution of bichromate. When dry it leaves a hard smooth surface. After a sufficient insolation under the negative, the paper is covered with lithographic ink, then immersed in cold water to dissolve out the unchanged albumen, which is then removed with a fine sponge.

Horsky's diffusion apparatus does away with the rasping process in the manufacture of beet-root sugar, dispenses with three-fourths of the manual labour, and extracts the saccharine matter completely. The yield of sugar obtained by the use of this arrangement has this season amounted to 8·5 per cent, an amount greatly superior to that obtained in establishments where other processes for extraction are in use.

The following is the formula for Dr. Jeannel's horticultural manure:—

Nitrate of ammonia..	400 parts.
Biphosphate of ammonia	300 ,,
Nitrate of potash	250 ,,
Hydrochlorate of ammonia	50 ,,
Sulphate of lime	60 ,,
Sulphate of iron	40 ,,

At a general meeting of the Société Francaise de Photographie, held on August 1, 1873, a letter from M. Anthony, of New York, was read, offering the following prizes, open to photographers of all nations:—100 dols. for the best bust of a lady; 100 dols. for the best head of a boy under six years of age; 100 dols. for the best head of a girl under six years of age; 100 dols. for the best group of two children under six years of age; 100 dols. for the best landscape. The proofs to be 16½ by 21½ centimetres, mounted on cards 25½ by 30½ centimetres.

At the same meeting M. Champion gave the following as the result of his experiments on the preparation of gun-cotton:—The acid mixture consists of 2 measures of nitric acid at 40° B., obtained by mixing common and fuming nitric acids, 3 measures of sulphuric acid at 66°. The mixture may be used either cold or at 40° C. The cotton is left in contact with the acid for three minutes, and the product washed till perfectly neutral.

Dr. E. Priwoznik records a change in cast-iron produced by the action of a mineral sulphur water. On examining an iron water-pipe which had been exposed for twelve years to the action of water rich in the sulphide of hydrogen, the innermost stratum was found to consist of—

Hydrated oxide of iron	81·08
Free sulphur	12·29
Sulphide of iron	4·48
Hygroscopic water	0·57
Nickel, cobalt, magnesia, silicic acid (soluble and insoluble), traces of carbon, and chlorides of ammonium and sodium	1·58
	100·00

This stratum is, therefore, an intimate mixture of hydrated oxide of iron, sulphide of iron, and sulphur. The hydrated oxide has the composition $2Fe_2O_3,3HO$, and is therefore identical with limonite. The middle stratum contained 79·2 per cent of metallic iron, and the exterior 92·6.

Interesting researches on the stroboscopic determination of the pitch of tones have been made by M. Mach. In the apparatus there is a cylinder which makes three revolutions in a second, and is divided into five octaves, At one end of it begins 10 bands, which, however, become more numerous and dense towards the other end, being there 320. To the axis of a syren is fixed a disc having equidistant radial slits of the same number as the holes in the syren-disc. The surface of the rotating cylinder is looked at through this slitted disc, while the syren tone is gradually raised. According to the stroboscopic principle the bands look distinct and at rest where there pass before the eye an equal number of them and of slits in the disc. If a scale of numbers of vibration be attached to the cylinder, the number of vibrations of the syren can be at once ascertained by observing the part corresponding to the distinct and still ring of the cylinder. One sees, however, distinct and at rest, not only the part of the cylinder corresponding to the number of vibrations of the syren, but also all those parts which correspond to the harmonic over tones. Of all such parts it is, of course, that one which furnishes the smallest number of vibrations that corresponds to the vibration-number of the syren. The determination may be varied in accuracy by varying the bands on the paper of the rotating cylinder. The apparatus may be applied to other sounding bodies. Thus let a mono-chord string be stretched at right angles to the axis of the cylinder; then simple teeth (Zachen) appear where the sounding string is opposite that part of the cylinder indicating the same number of vibrations. Another application is to attach small mirrors to tuning-forks, and watch in them the image of the rotating cylinder. An organ pipe may be also submitted to observation with aid of König's capsules and dancing jets.

ERRATA.—Page 474, footnote, for *plane* read *slane*. Page 480, line 22 from bottom, for *bog* read *boy*. Page 540, line 25 from bottom, for *north-western* read *south-western*.

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